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THE

NORTH AMERICAN
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VOL. VI.

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Eastern District of Pennsylvania, to wit:

BE IT REMEMBERED, that on the tenth day of October, in the fifty-third year of the independence of the United States of America, A.D. 1828, Hugh L. Hodge, Franklin Bache, Charles D. Meigs, Benjamin H. Coates, and René La Roche, of the said district, have deposited in this office the title of a book, the right whereof they claim as proprietors in the words following, to wit:

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D. CALDWELL,

Clerk of the Eastern District of Pennsylvania.

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THE
NORTH AMERICAN
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JULY 1828.

Original Communications.

Article I.—*Observations on Expansibility as a Vital Property, and on the Influence of the Capillary Tissue over the Circulation of the Blood.* *By* Hugh L. Hodge, M.D., *Lecturer on the Principles of Surgery in the Philadelphia Medical Institute.*

Inflammation has always engrossed the attention of pathologists. Its causes and nature have been the subjects of laborious investigation and of animated discussion. Former theories have however been generally abandoned, and in our day, the only important question much agitated regards the condition of the capillary tissue*; whether it be excited or depressed,—whether the capillary vessels be in a state of increased or diminished action during the inflammatory process.

Every one, acquainted with the science of medicine, must be aware of the importance of this question. All reasoning as to the symptoms, causes, and treatment of inflammation, is involved in its decision. The views taken of the modus operandi of the causes and remedies of this disease, must be predicated on its nature; and hence the practical principles of those who differ on this subject must be, so far as such individuals act consistently, directly opposed.

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* In this essay I shall use the expressions *capillary tissue* and *capillary vessels* as synonymous—believing that no point here discussed is essentially involved in the present anatomical investigations as to the structure of this tissue, whether it consist of minute vessels frequently inosculating, or whether it be a kind of delicate net work or cellular structure, to which the arteries and veins perform the offices of vasa afferentia and vasa efferentia. This is the idea of professor Broussais, professor Schultz, and others.

With Mr HUNTER, I believe that there is a morbid increased action of the capillaries in inflammation, in opposition to VACCA, ALLEN, LUBBOCK, PHILIP, HASTINGS and others, who maintain that there is a diminished action of the vessels, and that their enlargement is passive, owing to the *vis a tergo* derived from the heart and arteries. In every inquiry after truth on this subject, we should previously determine what is meant by the *action* of a capillary vessel; for on this point the whole discussion will turn. Dr PHILIP and his disciples employ this word as synonymous with contraction. Increased action of a vessel is with them increased contraction, followed, of course, by a diminution of size and expulsion of its contents. But Mr HUNTER affixed a different meaning to this word; for he speaks of an increased action of the vessels, and yet affirms that in inflammation they "do not contract*." This language is not peculiar to Mr HUNTER; for almost every medical writer uses the word action metaphorically, to express the state or function of a vessel, a tissue, or an organ. Hence we read of the action of the vessels in secreting bile, saliva, urine, &c.—of the action of the serous, or mucous tissue, of the iris, nipple, &c.—of the action of the stomach in digestion, of the brain in volition, &c.; not believing that there is in such cases any necessary contraction. The action, therefore, of a capillary vessel implies the manifestation of its vital properties,—increased action is the manifestation of such properties in an exalted degree.

The pathological question is thus reduced to a physiological inquiry as to the vital properties of capillaries; as to the modes in which their powers are displayed; and finally as to the consequences of the manifestations of their vital properties.

Upon many of these points there is no dispute. All physiologists agree, that capillaries have the vital property of contractility, that they contract on the application of certain stimuli, and that the result of this contraction is the expulsion of their contents. The object of this paper, however, is to show, that capillaries have a vital property of expansibility,—that they dilate under the influence of certain stimuli, and that the effect of their dilatation is to draw blood into their cavities from the supplying arteries, and thus materially to facilitate the motion of this fluid in the large as well as in the small vessels. The facts to be presented in support of this opinion, will render it at least probable, that expansibility is a property, not only of the capillary tissue intervening between arteries and veins, but of all capillary vessels,—of the radicles of the veins, lacteals, lymphatics, and ducts of glands, and that it is possessed in a slight degree by the heart and arteries, also as well as by the tissues confessedly erectile, as the penis, nipple, iris, &c.

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* HUNTER on the Blood, p. 251, American Edition.

While maintaining that certain tissues expand under the influence of stimuli, we are not obligated to demonstrate the mechanism of this operation, or even the tissue concerned. If the fact be proved, the lover of truth will receive it for the same reasons, and on the same grounds, as he believes in contractility, gravity, or electricity; and on reflection he will not find it more wonderful, that the particles of a living fibre should repel, than that they should attract each other. Contraction is as remarkable as expansion: *a priori* one could have been imagined as readily as the other. If we had been accustomed to regard expansibility as a vital property, this property would not appear more singular than that of contractility. Habit alone constitutes the difference in our minds. Who considers the repulsion between the north poles of two magnets, as more extraordinary than the attraction between a north and a south pole?—or who can explain the fact of attraction better than of repulsion? Both are equally incomprehensible*.

That expansibility is a vital property is not a new idea. BORDEU alludes continually to the erection of all glandular tissues as necessary for secretion, and to the erection of the absorbents as essential for their functions*. **Hunter** speaks of a " power of dilatation,"—of " an action of dilatation," and has been followed in the use of such language by many authors. The French and other continental pathologists acknowledge its existence in what they term the erectile tissue.

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* Were we disposed to wander from facts to hypothesis, the experiments of WILSON PHILIP, of PREVOST and DUMAS, of DUTROCHET, and others, manifest sufficient analogy between the nervous power and galvanism or electricity, to form an hypothesis for the explanation of vital contraction and expansion, as readily as of physical attraction and repulsion. Vide some ingenious observations on the agency of repulsion in physics by professor HARE in his Chemical Compendium.

The only author, however, who expressly acknowledges and treats of expansibility as a vital property is V. **Prus** in an octavo volume, " De l'Irritation et de la Phlegmasie," Paris, 1825. By this power he explains the afflux of fluids to irritated parts, but does not attribute to the capillaries any active influence over the general circulation. That the motion of the blood in the large vessels is accelerated by a suction power in the capillaries, is an opinion promulgated by professor **Smith** of Yale College, Connecticut, in the November and December numbers of the Philadelphia Monthly Journal for 1827; and by professor **Charles Schultz** of the University of Berlin, in **Meckel**'s Archives of Anatomy and Physiology for 1826†, and has been taught by me more or less fully, since my first course of lectures in the Philadelphia Medical Institute in 1823.

The paper of the Berlin professor I first read after writing this essay, and find him using the following explicit language on this subject. " The course of the blood through the arteries does not depend only on the impulse received from the heart; the peripheral system [so he names the capillary tissue] has an *equal influence* in drawing the blood to itself."—Journal des Progrés, vol. vii. p. 68, 1828‡.

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* Vide Dict. Abrégé des Sci. Méd., tome vii. art. Erection, &c.—North American Medical and Surgical Journal, vol. i. p. 416.

† His opinions on the formation and motion of the blood are fully reviewed in the 5th, 6th, and 7th volumes of the Journal des Progrés, &c. Paris, 1827—8.

‡ Drs **Carson** and **Barry** have contended for the suction influence of the thorax over the blood in the veins, and have hence been severely criticised by Dr N. **Arnott** in his Elements of Physics. Although not advocates for the peculiar opinions of these physiologists, we believe neither they nor ourselves are affected by the criticism of Dr **Arnott** respecting the influence of suction over the motion of the blood. He says " such a doctrine could not be proposed or entertained for a moment by a person, understanding the principle of a common household pump; and that it has been published and tolerated by able men in the present time, will remain a proof to posterity of the deficiency, as regards fundamental science or natural philosophy, which now exists in the ordinary medical education."—p. 507. " The veins are pliant tubes free to collapse, and no pump can lift liquid through such." " A practical illustration of this proposition

The points to be discussed will all be embraced in the three following propositions, which I shall endeavour to sustain by facts and arguments.

I. The capillary and arterial tissues do not dilate passively from a vis a tergo, derived from the heart and arteries.

II. Some tissues possess expansibility as a vital property; that is, are capable of expanding under the influence of certain stimuli. This property is possessed most remarkably by the capillary system intervening between the arteries and veins, and by the radicles of veins, of the absorbents, whether lacteal or lymphatic, and of the excretory ducts of glands. That it also exists in the heart and arteries, perhaps also in the veins and the ducts of glands, as well as in those structures denominated erectile, as the iris, penis, nipple, &c.

III. The circulation of the blood in the arteries is powerfully aided by this dilating power of the capillaries, drawing the blood into their cavities; and this power of the capillaries is the almost exclusive agent in all increased local determinations of blood.

IV. During the activity of inflammation; an increased quantity of blood flows through the affected part.

PROPOSITION I.

The Capillary and Arterial Tissues do not dilate passively from a vis a tergo, derived from the heart and arteries.

The hypothesis maintained by Dr Philip is, that the vessels will enlarge when the vis a tergo is increased, or when the resistance of the vessels, in consequence of exhaustion or debility, is diminished.

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is afforded by putting the point of a syringe, capable of making a complete vacuum if desired, into a piece of gut, or eel skin, or vein, filled with water, and then trying to pump up the water. The result will be, that the fluid close to the mouth of the syringe will enter it, and the sides of the pliant tube will then collapse as a valve against the syringe making an end of the experiment."—p. 508. This must be granted; but our accurate mechanician has forgotten one essential circumstance as regards the living system; namely, the vis a tergo, the momentum which the blood already has in the veins, and which would prevent the valvular collapse of their parietes. This fact was noticed by Dr Carson and ought not to have escaped Dr Arnott in his criticism. His illustration would have been more appropriate if two syringes had been employed, one at each extremity of a pliant and distended tube, so that the piston of one should descend while that of the other was slowly withdrawn. So far from a collapse, the velocity of the fluid would be increased by the suction power of the opposing syringe.

The dilatation is owing to a disproportion between the vis a tergo and the resistance of the vessels.

In support however of my first proposition, and in opposition to the opinion of Dr PHILIP, I present the following facts.

a. Contusions, when so severe as to paralyze the vessels without destroying their vitality, leave a part pale for a longer or shorter period according to the violence of the blow. BOYER in his Surgery (tome i. p. 105), notices several cases of local asphyxia from blows, "in which the heat, the sensibility, the movements, the pulsations of the arteries, in fine all the phenomena of life, after having been apparently annihilated for many days, gradually returned and resumed all their former vigour." He cites from DE LA MOTTE the case of a boy whose right hand was completely paralysed by a blow with a billiard stick, received on the outside of the forearm; "the hand for ten days appeared to be dead; but at the end of that time the heat gradually returned, the beating of the arteries became perceptible, and the hand was restored to its natural condition." How can the supposition of passive dilatation be consistent with these facts? The vis a tergo from the heart and arteries was perfect; other parts of the capillary system are dilated; but those vessels, whose powers are most exhausted, instead of being most dilated as they would be, if the theory were true, are the most constricted.

b. The same argument applies with equal force to all cases of local paralysis, whether from injury of the nerves of the part, of the spinal marrow, or of the brain. The vital strength of such parts is said to be weakened, and the vis a tergo from the heart is powerful; nevertheless, the large and small vessels diminish in size; they are not distended by an extrinsic force.

c. In every example of sedation, or diminution of excitement, the blood vessels of the part diminish, however active may be the general circulation. The mammæ lose their tension, and become soft and flabby in hysteritis; the feet become cold and bloodless in phrenitis; a wound of the scalp becomes pale, and the granulations and enlargement of the vessels disappear on the occurrence of severe arachnitis. In gangrene, the tension and swelling diminish before coagulation ensues, and before the vis a tergo has abated. Examples of this kind might be indefinitely multiplied.

d. Cold is very generally regarded as a sedative to the organic actions, even by those who maintain its stimulating influence on animal life. Parts subjected to its continued operation diminish in size; ligatures become loose, rings fall from the fingers, and paleness ensues. The weakened parts do not distend so long as the cold is operative, and never until reaction is induced by exercise, heat, or some other stimulus[*].

e. Parts which have been exhausted by continued irritation diminish in size; they are not distended by a vis a tergo as supposed by Dr Philip. A common example is observed at the orifice of an open bubo, and indeed of every chronic abscess. The edges are generally livid—the circulation continues but is very slow; the parts are incapable of adhesion or of granulating, and yet, no enlargement ensues from the influence of the vis a tergo on these exhausted parts.

f. When obstructions to the course of the blood exist, it has been supposed, that an enlargement and increased action of the vessels would result, giving rise to inflammation and fever. This idea has been the foundation of almost every theory of inflammation. Dr Philip at first adopted it in his work on Fevers (vol. ii. p. 23), but he, and most physiologists have abandoned the idea; for obstruction is universally succeeded by a diminution and not by an enlargement of the vessels, however strong the vis a tergo. Ligatures on arteries, pressure by bandages or tumours on these vessels, are followed by a diminution of size. So also when coagula form in the vessels, in aneurisms, mortification, &c. there is no dilatation; and when large congestions of blood occur in the venous vessels of the head, breast, or abdomen, the fulness of the arteries is lessened, and the pulse is small and weak.

The same is true of capillaries, as has been proved by the experiments of Spallanzani, Boyer, Philip, Thomson, Hastings, (p. 50), Gendrin[†], and others, all of whom affirm that the blood retrogrades from the point of obstruction; the vessels diminishing in size, and yet exposed to the vis a tergo.

g. Arteries are often found in gangrenous abscesses, deprived of their exterior support by the sloughing of the cellular membrane and other soft parts, and yet maintaining their vital functions. In no such instance have they enlarged passively from the impetus of the blood.

h. Mr J. Hunter exposed the femoral artery of a dog and dissected away its external cellular coat, on which the strength of the vessel is supposed to depend. No swelling was produced by the force of the circulation.

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[*] Vide Larrey's Campaigns.

[†] Review of Thomson on Inflammation, Journ. General. Aout 1827.

i. Mr **Jones** tied a ligature on the carotid artery, so firmly as to rupture the internal and middle coats, and to contuse severely the external tunic. The ligature was removed, and yet no dilatation ensued. Similar experiments were made by Mr **Dalrymple**, (**Travers**'s Essay, Medico-Chirurgical Trans. vol. iv. p. 442,) and by Mr **Hodgson**, (Diseases of the Arteries, p. 228,)—and with the same result as respects dilatation of the injured vessel. Hence a hernia of an artery is not regarded by modern surgeons as a possible occurrence.—(Vide North American Med. & Surg. Journ. vol. ii. p. 202.)

The above facts entitle me to the conclusion, that the vis a tergo, whatever may be its force and whatever impetus it may give to the blood, has not the effect of dilating the arterial or capillary tissue. To this conclusion, I know of but two apparent exceptions. The first is the formation and progress of true aneurisms; but all pathologists acknowledge the disorganized condition of the vessel in such cases. The second is the enlargement of the vessels during the systole of the left ventricle. This has been positively denied by **Parry**, after the most careful and varied experiments, but has been maintained by many careful observers*, and cannot be entirely denied. The dilatation however is trifling, and being confined almost exclusively to the pulmonary artery and aorta, forms, at the utmost, but a slight, and I believe, the only apparent exception to our proposition. As regards the capillary tissue I know of no example of passive dilatation from the impetus of the blood derived from the heart and arteries.

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* The latest writer on this subject is professor **Schultz** of Berlin, who affirms that " the dilatation of arteries, corresponding with the contraction of the heart, can be distinctly observed only in the great vessels, in the aorta and pulmonary artery; and this, not only in birds and the mammiferæ, but also in reptiles and fishes." " This dilatation of arterial trunks is only visible at a short distance from the heart; it disappears insensibly, its limit however is not well marked."—P. 69, Jour. des Progrés, &c., tome vii.

From the facts adduced under the second proposition, the question will arise whether this dilatation, trifling as it is, is simply the result of mechanical impulse.

PROPOSITION II.

Some Tissues possess Expansibility as a Vital Property; that is, are capable of expanding under the influence of certain stimuli.

The phenomena of expansion are so frequently observed in the nipple, penis, and other organs, and are in such cases so evidently independent of a vis a tergo, that the most distinguished modern physiologists have admitted, with **Bordeu**, an erectile tissue, and the consequent occurrence of vital erections. But as these active dilatations are occasionally observed in all parts of the body, and as they almost always involve either the absorption or expulsion of fluids, I am disposed to refer the expansile power to the *vascular* system, using this word in its most extensive sense*, without attempting however to demonstrate the tissue or tissues to which it belongs. My present object is to show that it exists,

1. In the *absorbents*, whether venous, lacteal, lymphatic, or glandular. Absorption is a vital process. It ceases with life. It is increased by appropriate stimuli, and diminished in cases of exhaustion or weakness. If contractility were the only vital property, absorption would be impossible under the influence of stimuli. The consequent contraction would exclude the stimulating fluid. In order that absorption may take place, the orifices of the vessels must be rendered patulous; that is, must dilate under the influence of appropriate stimuli. This is observed in all animated beings, so far as we can examine the phenomena occurring in such minute vessels as the radicles of the absorbents.

a. The seeds of vegetables, under favourable circumstances of soil, moisture, heat, &c. swell, enlarge, and imbibe fluids. The dilatation of their organic pores necessarily precedes the entrance of the fluids, and of course, the formation of the new plant, the result of the irritation.

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* This opinion appears to be supported by the following facts:—vital erections are observed in all organized tissues;—in all active dilatations, vessels are enlarged and fluids attracted into their cavities;—the degree of erection is proportionate to the vascularity of the part and to the mechanical facilities for distention arising from structure, location, &c.;—and finally, the minute anatomical investigations of those tissues, termed erectile, show, that their essential structure consists of vessels greatly convoluted and frequently anastomosing,—presenting when divided a cellular structure. They appear to be types of the capillary tissue. "To explain its [the iris] singular movements, there is no necessity for admitting the existence of contractile muscular fibres; it is only necessary to notice the eminently *vascular and nervous* texture of the iris which gives to it (qui le fait jouir,) an active expansibility." M. **Jourdan**, Dict. des Sciences Médicales.—**Prus**, p. 39.

b. The same phenomena are observable in the roots of living plants, the seat of absorption. All their vessels are distended under the influence of stimuli, and hence filled with fluids, affording even to the unscientific observer, a striking contrast to the comparatively dry and constricted roots of a dead vegetable.

c. When plants are propagated by slips, or engrafting, the same turgescence of the absorbing vessels is manifested.

d. In animals, the mouths of the absorbents are so minute and the vessels so situated, that ocular investigation is usually impracticable. But visible absorbent orifices are presented to us in the puncta lachrymalia. Their operation is vital; for no artificial puncta have ever answered as a substitute, and when they become weakened by disease or old age, there ensues a stillicidium lachrymarum, however mechanically pervious they may remain, or however suitably located. When under the influence of their normal stimulus, the tears, they absorb this fluid from the tunica conjunctiva. This excitement is manifested, not by contraction, for then the tears would be excluded, but by expansion, evident to the eye and essential to their functions.

e. Certain stimuli increase absorption. Surgeons daily recommend frictions, irritating liniments, blisters, &c. for this purpose, and with acknowledged advantage in the discussion of fluids and tumours. If the influence of such remedies were to excite contraction, absorption would be diminished, not facilitated.

f. The lactiferous tubes of the female mammæ dilate under the influence of certain stimuli. Every one is aware of the readiness with which the milk flows during the pleasing irritation to which the nipple is subjected by the infant's mouth. That the discharge is not solely produced by suction, is proved, 1st, By the common operation of milking, in which the tubes emptied by pressure dilate,and hence absorb more fluid from the gland, under the influence of the irritation; 2d, By the fact that animals have some power over the excretion of milk, to such an extent at least, as not to yield it when they are irritated, or when they are mourning for their young; 3d, By the discharge caused by the application of irritants to the nipple without any mechanical assistance*. In several instances I have witnessed an immediate flow of milk on touching the nipple with lunar caustic;—the tubes enlarging under the irritation, and giving exit to the liquid previously retained. The dilatation was immediate, and being essential to an important function, cannot reasonably be attributed to relaxation from prior stimulation.

The power thus visibly demonstrated in the absorbents of the tears and in the lactiferous ducts must, for the same reasons, be possessed by all other absorbents, and by all excretory ducts. They all have to take up fluids when appropriately stimulated, and must therefore dilate; for contraction alone would be inconsistent with their functions, and stimuli would render absorption more tedious instead of increasing its activity. In all these examples of dilatation, there is no vis a tergo operative, and hence the admission of the enlargement of absorbing vessels would be conclusive as to the active nature of such dilatations.

2. *Capillary blood vessels* possess Expansibility.

g. Certain stimuli or irritants† are immediately followed by an enlargement of the capillaries; there is no evidence of prior contraction and subsequent exhaustion.

Caloric is a universal stimulus. When a portion of the cutaneous tissue is immersed in warm air or warm water, an immediate distention of the capillary vessels is observed. This dilatation by the stimulus of heat is as immediate as the contraction of a muscular fibre from any mechanical irritant; the one is as direct an effect as the other. There is no evidence of exhaustion or debility of the capillary vessels; for all the phenomena peculiar to life are augmented, such as heat, scarlet redness, circulation, and secretion. Neither should the effect be attributed to an increase of the vis a tergo; for, 1st, The enlargement of the capillaries occurs before the excitement of the supplying arteries; 2d, It is often great when there is no evidence of increased action of the larger vessels; and 3d, Such dilatations may often be excited by caloric, when the actions of the heart and arteries, including the supplying arteries, are exceedingly depressed. In all cases of prostration, where the pulsations of the heart and arteries can scarcely be perceived, where the surface is cold and pale, and all the organic vital phenomena are greatly diminished, warm applications are always resorted to; and when they redden the skin and produce inflammation or vesication they are advantageous. Where in such cases is the vis a tergo to dilate the capillaries? or what reason is there for imagining that the effect of the stimulus on these pale, bloodless, and weakened vessels, is to contract them, and thus to exhaust their power that they might yield to a vis a tergo? Further exhaustion would be death, which caloric, as every practitioner knows, would rather prevent than accelerate.

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* Prus, Op. cit. p. 38.

† When not too intense or acrid, for in such cases vital power is overwhelmed. A glass of brandy stimulates; a pint of alcohol kills instantaneously. Warm water at 98° of Fahr. increases vital action; iron at a red heat destroys the part to which it is applied.

These remarks respecting caloric may be extended to most other irritants. The puncture of a needle, or the sting of a wasp, is immediately followed by redness; a mote under the eyelids instantaneously causes an enlargement of the vessels of the conjunctiva; a drop of nitric acid reddens the skin, and so of other irritants. In none of these cases is there evidence of contraction preceding dilatation; or of exhaustion directly or indirectly produced*.

h. Internal stimuli often enlarge the capillaries without any evidence of an increased vis a tergo. How instantaneous is the appearance of a blush, mantling the face and neck from the operation of certain passions. The enlargement of the small vessels in blushing is the result of a nervous irritation, not of increased action of the heart; for 1st, This viscus is often undisturbed: 2d, In all instances the blush is partial; if owing to the contraction of the heart it would be universal: 3d, In violent palpitations and other increased actions of the heart, the capillaries are not distended as in blushing, but are often remarkably contracted. Their enlargement, as well as their contraction, must depend on inherent powers.

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* There can, I think, be no more decided proof of the inaccuracy of microscopical experiments on the blood vessels of living animals, or of their inadequacy to support the pathological doctrines erected on them as their sole foundation, than the assertion of Mr HASTINGS, as deduced from experiments (p. 55, HASTINGS on Bronchitis, London, 1820), that the direct effects of heat and cold are the same; both producing primarily active contraction, and secondarily, debility, allowing of passive dilatation from a vis a tergo. If however Mr HASTINGS would place one hand on a mass of ice, and the other near a fire, the opposing influences of heat and cold would be visibly demonstrated. Vide a critical examination of the experiments and opinions of Drs PHILIP and HASTINGS in the introductory volume to the posthumous works of Dr CALEB H. PARRY, by his son CHARLES H. PARRY, London, 1825.

i. The remedial agents, which increase the diameters, as well as the functions of capillaries, without augmenting the action of the heart and arteries, in other words, the vis a tergo, are very numerous. To this head must be referred most of the diaphoretics, diuretics, expectorants, and cathartics. Mercury also will often produce local dilatations, secretions, and even inflammations of the skin, gums, tongue, and salivary glands, without any evident excitement of the general circulation. In such cases, is not the hypothesis unfounded which maintains that the vessels, thus acted on by internal stimuli, and having all their vital phenomena increased, must be debilitated before they enlarge?

Even the dilatations of capillaries from the diffusible stimuli, wine, brandy, &c. which do disturb the heart and arteries, cannot be owing to a mere mechanical impulse; for, as already hinted, the enlargement is often in an inverse ratio to the vis a tergo. Thus, when the action of the heart is violent, the capillaries are often undistended; and their distention is often very great when the cardiac pulsations are languid.

k. Again: Whenever any excitement is manifested in the functions of a tissue, organ, or apparatus; or when any new or increased demand is made on such tissues or organs, the capillary vessels dilate to meet the emergency.

Compare the different conditions of the capillary system in childhood and old age. In the former all is activity, not only to replenish the various secretions and excretions so abundant in infancy, but especially to furnish the materials for growth. The capillary system is full and tense. In old age, the phenomena of life are diminished, nutrition and secretion are lessened; while wrinkles and emaciation are substituted for the fulness and tension of a prior stage of existence. The same difference is observed in the peripheral system of infants when in health and when diseased; and of the extremities before and after the occurrence of paralysis. Where the phenomena of life are most displayed, the capillaries are most developed; where they are diminished, these vital tubes are proportionally smaller. Surely in the former case, they are not debilitated and passively dilated.

The same remarks are applicable to all *general* excitements of the capillary system. Are not the vital energies in greater exercise, and the capillaries at the same time more dilated in an inflammatory fever than in a typhous fever, or in hectic wastings; during the ruddy excitement of health than during syncope; in the hot than in the cold stage of fever; during the stage of reaction than in that of collapse from concussion of the brain; when under the influence of heat than of cold; while the individual is burning with anger than when torpid with fear? That the capillaries in these and similar cases are not distended by an increased energy of the heart was proved in our first proposition.

The same enlargement of the capillaries is observed in all cases of *local* excitement: cases in which no one contends that there is a diminution of any of the vital phenomena. The capillary vessels are enlarged in the growth of tumours; in tissues or glands for their increased secretion, whether temporary or continued; in the heads of animals which are renewing their horns; in the organs of generation of those female animals which are in season; in the brain and face, during excitement from moral or physical causes; in the uterus after conception, and even before the descent of the ovum; in the iris, nipple, and penis when appropriately excited. In none of these cases is there any evidence of exhaustion of power or an increased vis a tergo. In all, the manifestations of vital energy are increased.

l. The active enlargement of capillaries is evinced when the main artery of a limb has been tied. The immediate effect is the cessation of the circulation below the ligature, and the consequent coldness and paleness of the limb; but in a few hours the circulation usually returns, and the thermometrical temperature of the affected limb and the activity of the capillary circulation are greater than those of the sound limb. This excitement after some time gradually diminishes to the ordinary standard of health. The increased activity of the capillaries has been referred to the vis a tergo, operating through the anastomosing vessels; but this explanation is not satisfactory for several reasons. 1st, The influence of the heart's contraction, when the blood passes through small anastomosing vessels and by circuitous courses, must be less powerful than when the main channels remained pervious. The vis a tergo is therefore lessened, and yet the capillary distention is greater than it was before the operation, or than in the sound limb. 2d, This argument is of more force, when it is considered that the enlargement commences in the smaller, and gradually extends to the larger arteries; and also that in some cases the branches from the main artery, through which most of the blood must pass, do not become percepsibly enlarged. For these anatomical facts, we have the best authority. Mr HODGSON, on Diseases of Arteries, p. 247, says "the dilatation takes place principally in minute ramifications; the trunks and the mouths of the vessels going off above the place of obstruction in the main artery, in several preparations, did not appear to be larger than in their natural state, and in a few instances only, a slight dilatation was perceptible."—Again: "the dilatation of the minute arteries does not appear to be owing to the vis a tergo when the main artery is tied. After amputation, the main artery and its branches are not enlarged; I have invariably found them remarkably contracted." Professor SCHULTZ observes (Journal des Progrès, &c. vol. vii. p. 62), "this dilatation [of the anastomosing vessels] does not occur immediately after the operation: it is slowly effected. M. DE WALTHER has even affirmed that he could not discover any dilatation of the collateral vessels after the death of a man who had suffered from an aneurism at the bend of the arm." From these facts, it is evident that the vessels, which are the least under the influence of the heart, are the most dilated. 3d, That the dilatation is not owing to the vis a tergo is also proved by the eventual return of the capillary, and of the more minute, arteries to their ordinary size, at the time when the larger branches being dilated, the vis a tergo becomes more operative.

Let these facts be taken in conjunction with those in support of proposition first, and it can hardly be affirmed that the increased size of the capillaries after obstructions of the main artery, is owing to a vis a tergo. I believe that in such cases, the stimulus of arterial blood being withdrawn on the application of the ligature, a temporary suspension of action ensues. This is followed by reaction, agreeably to a law of the animal system and as observed under various other circumstances, as when the hot succeeds the cold stage in fevers; during which excitement the capillaries dilate and draw the blood freely into their cavities, as will be fully illustrated under our third proposition.

The facts hitherto adduced are chiefly *physiological*, demonstrating, I think, that expansibility is an inherent property of the sanguine capillary tissue. *Pathological* facts bear with equal force on the same point, and may be confidently urged, especially upon those who regard physiology as the true basis of pathology. Let it be remembered that the question to be determined is, whether the enlargement of the capillaries is active or passive;—produced by an inherent dilating power, or by a vis a tergo.

m. During the acute stages of inflammation, the parts are very tense and hard ; and moreover, when such parts are covered by any unyielding membrane, as hardened cuticle, fascia, periosteum, &c., the phenomena of inflammation are greatly enhanced, and the progress of the disease to suppuration or gangrene is hastened, while the fascia or membrane becomes very tense. Whence the distending power in such cases? Is it intrinsic or extrinsic?—Not extrinsic, because 1st, The distention is not proportioned to the excitement of the heart and arteries ; being often great when this excitement is at the healthy standard, and sometimes even when the general circulation is greatly depressed. 2d, If the swelling and tension were owing to a vis a tergo, the resistance of the membranes, so far from aggravating, ought to cure the inflammation by restoring the equilibrium between the resistance of the small vessels and the vis a tergo. The passive distention of the former would be effectually resisted and the balance restored, which, according to the seductive simplicity of the *mechanical hypothesis* of Dr Philip, is the only desideratum in the treatment of inflammation. Hence Dr Lucas, one of his disciples, recommends pressure as one of the most powerful remedies for this disease ; but every practical man knows the mischief produced by pressure from bandages, ligatures, tendinous sheaths, fasciæ, &c. during the acute stages of the phlegmasiæ ; for 3d, We find that the removal of pressure moderates the symptoms, shortens the disease, and facilitates resolution. Hence one advantage derived from fomentations and poultices in superficial inflammations in softening the indurated cuticle ; hence the necessity for dividing in a transverse direction the tendinous sheath in paronychia and the fibres of fasciæ during sub-aponeurotic inflammations. Relief could not be afforded by such measures, if the dilatation were passive ; for the vis a tergo being increased and the capillaries being debilitated, the distention would continue augmenting until rupture or mortification should ensue.

This objection alone, I regard as sufficient to overthrow the whole hypothesis of our opponents. When inflammation disappears, they say the balance is restored between the capillaries and the supplying vessels,—the disproportion between the resistance and the vis a tergo having ceased ; but how can this ever occur spontaneously, that is, without the interference of the surgeon. The more the palsied capillaries are distended, the more incapable are they rendered of resisting the impetus from the supplying vessels.

How then is the supposed necessary balance restored? How can the capillaries recover sufficient energy to resist an impetus, which on their supposition is continually increasing? Rupture or gangrene we should imagine must be the result of every inflammation, in which art does not interfere with its pressure or its astringents.

n. The distending power not being therefore extrinsic, depending on a vis a tergo, is *intrinsic*, and its exercise is the result of irritation. For experience shows that when the irritation is removed, the tension disappears: the same capillaries exposed to the same increased vis a tergo, will often diminish in size on the removal of the cause. Remove a mote from the conjunctiva, or a splinter from the skin, and the enlarged vessels are lessened, and the inflammation is resolved, notwithstanding the excited condition of the supplying vessels. On the contrary, the increase of irritation is necessarily followed by a more violent inflammation; its phenomena becoming more intense in proportion to the severity of the irritant, until the vital powers of the part become exhausted and gangrene ensues. Irritation, therefore, and not the vis a tergo, is the cause of the tension of inflamed parts. This irritation is generally increased by pressure, which hence aggravates the phenomena and accelerates the progress of inflammatory affections.

o. Inflammation can be excited by sinapisms and blisters in patients whose vital powers and actions are nearly exhausted, when the pulse is not to be felt, and the surface is cold. The irritation is applied to capillaries greatly debilitated, and no practitioner acts on the supposition that such stimuli would still further weaken them; for then gangrene would ensue. Neither can there be, in such cases, any increased vis a tergo; yet they become enlarged—certainly by an inherent power excited by the irritants. So also the occurrence of "critical inflammations and abscesses" in prostrated states of the system, where the action of the heart and arteries is barely sufficient for the maintenance of life, is inconsistent with the idea of a vis a tergo as the cause of the dilatation.

p. The veins of an inflamed part and those proceeding from it are distended*. Whence this enlargement? Dr Philip and his supporters maintain that the blood is congested in the capillaries; that owing to the debility of these vessels, it is nearly or altogether stagnant. These tubes, therefore, cannot empty their own cavities; how then can they fill and distend the veins proceeding from them? If, however, the capillaries manifest, during inflammation, an increase of their physiological power, if they dilate actively, the blood will not only be determined to these vessels in larger quantities, but will traverse them more readily, and distend the veins from the impetus acquired from the heart and arteries, assisted in all probability by a contraction of the capillaries alternating with their dilatation. These points will be noticed under our third and fourth propositions.

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* "This was evident in the following case: I applied caustic to the ball of the great toe of a patient every other day; and after each application the surrounding part put on a blush, and all the veins on top of the foot as well as up the leg, immediately began to swell and became large and full. This was so remark-

q. The condition of the capillary vessels in acute dropsics and active hemorrhages, (states very analogous to inflammation, if not, as maintained by many, identically the same) also manifests the connection between vascular irritation and increased diameter. Such effusions of serum and blood often depend on local irritations without any increased vis a tergo, and sometimes even when such force is evidently diminished. The vessels instead of contracting, dilate under the irritation, allowing and facilitating the exit of the fluids; and it is only when the irritation is removed that the discharge is arrested.

r.. Nævi materni, and aneurisms from anastomosis afford very beautiful illustrations of the active independent power of the capillaries to enlarge their diameters.

3d. The *Arteries* also enjoy an expansile power. This is less evident than in the capillaries, but may nevertheless be demonstrated by attention to their varying physiological and pathological condition. That arteries do vary in their diameter, no one will deny, and in Proposition I., I endeavoured to prove that their enlargement did not result from the contraction of the heart. The vis a tergo is not sufficient to distend these tubes when paralysed, contused, or otherwise debilitated, much less when in the entire possession of their vital powers.

The physiological facts in proof of the active dilating power of arteries are very numerous. I can only allude to them under a few general heads.

s. The continued operation of local stimuli, causing first dilatation of the capillaries, speedily excites a sympathetic enlargement of

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able that the patient watched for this effect on the days on which the caustic was applied."—HUNTER on the Blood, &c., p. 137, Am. Ed.

the supplying arteries. Place a hand or foot in water at 100 deg. Fahr.; the distention of the capillaries will soon be followed by an increased fulness of the radial and ulnar, or of the tibial arteries. So also the supplying vessels dilate during the renewal of the stag's horns, the erection of the penis, the enlargement of the uterus in gestation, the distention of the mammæ in lactation, &c. &c. (Proposition II. *k*.) The pulse in such instances is not only fuller, but more tense and vigorous. This fulness and tension cannot be owing to the heart, for its actions are undisturbed; and the pulse in every other unexcited part is soft and of its natural volume.

t. So also we have a fulness of the pulse and an enlargement of the arterial system in all healthy excitations of the body, as from exercise, from the influence of hot air and hot bathing, from the exhibition of wine, brandy, and other diffusible stimuli, and from the operation of anger and other exciting passions. It can hardly be imagined that any modern physiologist should adhere so tenaciously to the mechanical philosophy of ages long since past, as to attribute such distentions of the arterial system to the forcible contraction of the heart. Neither will any one suppose that the arteries are debilitated when all their vital functions are augmented.

It would be extending this essay to an unreasonable length, to dwell on more detailed examples of the same normal enlargement of the arteries. I shall therefore merely adduce a few pathological facts confirmatory of the same views.

u. The arteries going to an inflamed part are enlarged. Dr Philip denies their enlargement; but Mr Hastings, his best advocate, does not question their increase of diameter. In cases of severe inflammation of any organ or tissue, the size of the supplying arteries will be found augmented. Hence the fulness of the carotids in phrenitis and tonsillitis, of the radial and ulnar arteries in rheumatic affections of the hand, &c. Whence their distention? For reasons already advanced (section *s*), not from an increased vis a tergo: neither is it from exhaustion or debility, for Dr Philip himself acknowledges their increased action. This fact indeed forms the very foundation of his theory; for debility of the capillaries with increased action of the supplying vessels constitutes his doctrine of inflammation. The power of distention belongs, therefore, to the arteries themselves. The irritation commencing in the capillaries is sympathetically propagated to the larger vessels, which idea is confirmed by the fact, noticed by Dr **James** and others, that if any of the supplying vessels be divided, others will enlarge.

v. This sanguine irritation often excites sympathetically an inflammatory fever. This state is characterized by an " increased action of the heart and arteries." The pulse of most, if not of all, tangible arteries, is full as well as tense, and a general turgescence of the arterial and capillary systems would seem to exist. Debility of the arteries is not here contended for, and the increased action of the heart is surely not adequate for the universal distention of the arterial and peripheral system of vessels.—Vide Proposition I.

w. A similar enlargement of the arteries is occasionally observed in acute hemorrhages, evidently depending upon the same causes.

x. When the main artery of a limb is tied, we have already noticed the fact (section *l*), that not only the capillaries but also the arteries are enlarged, without any debility of the vessel, and under a diminished vis a tergo.

y. In aneurismal varix, and in varicose aneurism at the bend of the arm, where part of the blood is immediately returned by the injured vein, and of course lost as respects the capillary vessels, the brachial artery is found enlarged. There is no increased vis a tergo, and there is no debility of the vessel. Its enlargement, therefore, must be active, that is, dependent on its own vital properties,—the irritation arising indirectly from the want of blood in the lower part of the limb. The permanent enlargement, says Mr **Hodgson***, seems to depend on *that property in the vascular system*, whereby the size of the vessel is always accommodated to that of the part which it supplies (that is to the want of blood in any part); hence the vessels enlarge in diseases of the joints, in tumours, &c., as well as after an artery is obstructed†. Mr **Hodgson** as well as Mr **Hunter** frequently acknowledge, in this indirect manner, the dilating power of arteries and capillaries, without expressly attributing to them expansibility.

z. Numerous experiments upon blood vessels made by distinguished physiologists, with other objects in view, prove the expansile power of arteries. I shall quote facts from two authors only, each of whom maintains opinions opposed to those which I am advocating. Mr Caleb H. Parry*, in his 23d experiment, after having exposed and carefully measured both carotid arteries, tied one vessel: the other increased in size. When this was also tied, it resumed its original dimensions; proving that the previous distention was not owing to a vis a tergo. To this conclusion, Parry himself was led; for he maintains as the result of numerous experiments, that the arteries are always during health distended beyond the size to which their elasticity would bring them (p. 69); and moreover, that this forced distention of an artery does not depend on the impulse of the blood from the left ventricle, (p. 71). This distention, he observes, is increased in local determinations of blood; and as it does not depend on the elasticity of the arteries, nor on the systole of the heart, "we are compelled to admit as the cause of the common dilatation of the larger arteries, and still more forcibly of that which is preternatural, the mechanical distending power of the blood which they contain."—p. 78. This explanation, however, is unsupported by facts; and it would be difficult to conceive how the blood, when in rapid motion through the arteries, could mechanically dilate one vessel and no other, unless some change previously occurred in the parietes of the tube.

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* Diseases of Arteries, p. 254.

† A respected friend has objected, that this and other analogous cases are examples of growth ; the enlarged size of the vessels arising from the deposition of new organic particles. I answer, that, in most cases, the enlargement is immediate, and that in all, dilatation must precede growth.

The second author to whom I refer for facts, is Mr Hastings†, who, after many and diversified trials, asserts that mechanical and chemical irritants applied to the larger arteries will often excite contractions and dilatations of the vessels in the part irritated. "This increase of motion did not arise from any modification of the heart's action; for the degree of dilatation and contraction in the unirritated part of the vessel nearer the heart was unaltered"—p. 26. Neither is the dilatation owing, as Mr Hastings would seem to intimate at page 31, to a diminution of irritability or to a suspension of power, for contraction would then be suspended instead of being augmented; nor to relaxation after prior contraction, for such alternate relaxations and contractions do not occur in muscular fibres (those of the heart alone excepted, to which we shall presently allude) when mechanically or chemically irritated. A tonic contraction exists, so long as the irritant is operative. The increased pulsation of arteries under the direct influence of stimuli, is, therefore, but a more clear manifestation of their normal actions ;—dilatation being the result of stimulation as evidently as contraction. This is a very fruitful subject. It embraces all questions relative to the causes, nature, and varieties of the pulse, and admits of numerous illustrations. But the facts already adduced, taken in conjunction with the well known varying condition of the pulse in cases of sedation or diminished excitement of parts, as well as in those of irritation, prove that its condition, as to size, tension, and strength, does not depend solely on the systole of the left ventricle, but, at least to an equal extent, on the degree of excitement in the arteries; and that the actions of the arteries are greatly influenced sympathetically by the ever-varying conditions of the capillary tissue to which they are distributed. The formation of the pulse depends on the heart; but its size and tension, on the degree of excitement existing in the arteries and capillaries, so long as there is a free supply of blood.

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* Parry on the Pulse, Ed. 1816.

† On Bronchitis, London, 1820, p. 24, et seq.

4th. Expansibility is exhibited by the *Heart*. The enlargement of the cavities of this organ after contraction has something so peculiar, that many physiologists have attributed it to an inherent power and not to a vis a tergo. From the Dict. des Sci. Médicales Abrégé, we learn that this opinion has been advanced by HAMBERGER, PERRAULT, SENAC, BICHAT, and DUMAS. Dr PHILIP, whose opinions we have hitherto been opposing, advocated this idea in his Inquiry, and since in the 12th volume of the Medico-Chirurgical Transactions; and it has been received with certain modifications by MAYO, BLUMENBACH, BRODIE, CARSON, BOSTOCK, PRUS, and others. Some of these authors refer the dilatation to a *vital* relaxation; some to a peculiar set of contractile fibres; others, as BOSTOCK, to elasticity; others to the relaxation of spiral muscular fibres; Dr CARSON to the dilating power of the lungs and thorax; and a few to an active inherent power, without locating it in any individual tissue of the heart. The first question, and the only one which at present concerns our subject, regards the actual existence of expansibility. This seems to be proved by the following facts.

aa. The heart of a living animal, when removed from the body, will continue to pulsate for some time. If the heart of a frog, just extracted, be thrown into warm water, a violent action, according to Dr CARSON, will be excited, and in some instances a small stream of a bloody colour will be projected through the transparent fluid. Dr **Philip** records the same fact.

bb. If the heart *in situ* be grasped with the hands, great resistance will be felt during its diastole. **Pæchlin** says a very powerful pressure cannot completely restrain this dilating power*.

cc. This dilatation continues, says **Prus**, after the veins have been tied; of course, therefore, in opposition to atmospheric pressure, as the entrance of blood is thus prevented.

dd. Mr **Hunter** (p. 125, Op. cit.) says that the heart dilates and contracts more fully from exercise and other natural excitants. The same stimulus which increases its contractions, augments its dilatations. This is a phenomenon not observable in the muscles of the trunk and limbs; of the stomach, bowels, bladder, &c., and one which the experiments of **Hastings** indicate as belonging also to the arterial system (section *z*); thus characterizing the circulatory apparatus and apparently essential for the motion of fluids.

The heart would hence appear to possess an active dilating power, exercising a slight suction influence over the blood in the large veins near this organ, and thus facilitating, instead of opposing, the ingress of the blood. Evidence, I think, is yet wanting to prove, that any part of the circulatory apparatus, during its healthy condition, resists the motion of the blood. Each portion co-operates, and beautifully harmonizes with every other portion, in carrying the vital fluid through the circulation,—an idea to be further illustrated in the ensuing proposition.

PROPOSITION III.

The circulation of the blood in the arteries is powerfully aided by this dilating power of the capillaries, drawing the blood into their cavities; and this power of the capillaries is the almost exclusive agent in all increased local determinations of blood.

"Ubi irritatio, ibi affluxus."

If the two previous propositions have been substantiated by the facts adduced, the present would result as an inevitable corollary. If the capillaries actively dilate, any fluid in the supplying arteries will necessarily be drawn into their cavities, on the same principle that a fluid enters a pump or syringe on the ascent of the piston; for, as already observed (p. 4, *note*), collapse of the sides of the pliant arterial tubes is prevented by the vis a tergo.

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* **Prus**, p. 51.

The suction power of the capillary tissue, which can thus be maintained by *a priori* reasoning, may, I think, be positively demonstrated by numerous facts,—proving not only its existence, but its great and essential influence over the motion of the blood; for although the suction exercised by any single vessel is exceedingly slight, yet the combined influence of the whole capillary tissue, forming the substance of every portion of the body, must be very effective, and "at least equal to the impetus received from the heart." Should also any portion of the peripheral system be excited, such excitement, causing increased dilatation, will simultaneously produce an increased afflux of blood to the irritated part. In adducing facts in support of this proposition, some repetitions will be unavoidable; but further evidence will be exhibited, while examining the circulation in all living beings, of the existence and importance of an active dilating power.

1st. *In Vegetables.*—The fluids absorbed by the roots are conveyed, under the name of sap, through tubes in the alburnum, to the branches, twigs, leaves, &c., and, after having suffered some change, analogous to that from respiration in animals, are returned by other vessels for the nutriment of the plant. The motion of the sap depends on vital powers; for it occurs only during the life of the plant, and in those parts only which retain their vitality. That expansibility, as well as contractility, is essential to the motion of these fluids, is shown by the following facts.

a. The sap ascends when the trunk and branches are subjected to the influence of a warm atmosphere; but it remains in the root when such stimulus is not applied. It is manifest, that the caloric, in such cases, operates on the superior or exposed portions of the plant; the roots, buried in the soil, being comparatively unaffected by atmospheric variations of temperature. Examination proves, that parts thus stimulated swell, and, from a comparatively dry state, become moist and succulent; in other words, that the vessels dilate under irritation, attract or suck in the nutritious fluids, whence result leaves, buds, blossoms, and fruit. There is no vis a tergo to distend the whole peripheral system of vessels, and in such textures, dilatation must precede the entrance of fluids.

b. This phenomenon and this explanation are more evident in the following fact. A vine, running on the wall of a smith's shop, shoots some of its branches through a crevice into the interior of the building. In winter the exposed portions became torpid and apparently dead; the ramifications, however, at the superior portion of the plant, which were warmed by the furnace of the smith, vegetate and blossom*. Its vessels, stimulated by caloric, attract from the distant root and through a torpid trunk, its nutritious juices.

c. HALES has shown, that if a branch be cut from a vine, and its cut extremity be immersed in a fluid, absorption ensues. In this case the ligneous tubes, not capable of any immediate alterations of their calibre, cannot be the active agents of absorption; the attractive power is located in the smaller vessels, and by its exercise, fluids are drawn into their cavities. Every one knows how long the beauty and vigour of leaves and flowers may be preserved by immersing their stalks in water.

d. An analogous fact is evinced in the propagation of plants by slips. Here nutritious fluids are absorbed from the soil by vessels which previously attracted their fluids from the roots of the parent plant. The success of the common operation of engrafting is to be attributed to the same suction power in the extreme vessels.

e. The course of the sap in plants is often altered by local irritations, so that the descending sap ascends, or the ascending descends. Thus M. DUTROCHET† affirms, from numerous observations and experiments, that the descending or nutritive sap becomes ascending in buds, in the peduncles of fruits, &c. He records, among others, the following fact. A vertical root of a tree was accidentally exposed for a considerable extent; the inferior extremity remained covered with earth, and a small sucker was observed at a short distance from the trunk. This root was divided during the winter below the sucker, and from this time was deprived of the ascending or lymphatic sap; it nevertheless vegetated and increased in size. This proves that the lymphatic sap descended from the trunk for its nourishment; for other roots under the same circumstances, but deprived of the bud or sucker, rapidly dried. Hence, M. DUTROCHET justly concludes, that the flow of the sap in a direction contrary to what it had originally pursued, was owing to the vital action of the sucker. This vital action, on whatever it may depend, whether on electricity, nervous fluid, or any other source, was certainly manifested by an enlargement of vessels and the consequent attraction of fluids.

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* This fact is recorded by RICHERAND in his Physiology, (American edition, p. 13) who states that it occurred under his own observation.

† L'Agent immediat du mouvement vital, &c. par M. DUTROCHET, Paris. Vide Review of this work, in the present number of this Journal.

f. Another fact in the physiology of vegetables tends to the same conclusion. When the branch of a tree is cut off, the stump dies, provided no branch, twig or bud exists between its extremity and the trunk; showing that the vis a tergo from the roots and other parts will not continue the flow of sap even in living tubes, unless assisted by an attractive power in the peripheral system of vessels*.

From the above statement it results that the motion of fluids in vegetables is influenced in a very important manner by a dilating power chiefly resident in the capillary tissue of their roots and branches. Expansibility is requisite not only for the absorption of fluids, (Proposition II. *b*, *c*); but also for the distribution of these fluids to every portion of the plant in proportion to the degree of local excitement. The capillary tissue of the roots and that of the branches harmonize for the accomplishment of an object, for which either separately is inadequate.

2d. *In Animals.* As we ascend in the scale of animated existence, the vital functions become more numerous and complicated. The lower order of animals, *the invertebral*, differ very little from vegetables in the mode of their existence, absorbing and circulating their nutritious fluids apparently by the same powers and a similar apparatus. The *vertebral* animals, forming the second of Cuvier's divisions, have an additional organ, more or less perfect, for increasing the impetus of their fluids. This new organ, the heart, is given, not as a substitute for capillary influences, but as an assistant, demanded by the increasing wants of the system.

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* That there is a vis a tergo operative in the circulation of the sap every one will admit. M. DUTROCHET has devoted much attention to ascertain satisfactorily its existence and source. He alludes to the experiment of HALES, repeated and confirmed by MM. CHEVREUL and MIRBEL, of adapting a glass tube, partially filled with mercury, to the cut extremity of a truncated vine; the mercury was elevated 30 inches above its primitive level by the force which expels the sap. To ascertain the seat of this impulsive force, M. DUTROCHET cut off in succession different portions of the branches and trunk of a vine in which the lymphatic sap was abundant. He found that the flow of sap was immediately arrested in the part detached, but continued in the part attached to the root. Hence he inferred that the power resided in the root. Having removed the earth around the root, transverse sections were repeatedly made, and he found that the sap always issued from the inferior surface. The sections were continued until the smallest radicles (le chevelu) were divided with the same result, proving that the impulsive force given to the sap originated in these minute portions of the root; and that the spongioles, or extreme conical terminations of the capillary radicles, hitherto regarded as the agents of absorption only, ought also to be regarded as the origin of the expulsive force of the sap.

It is needless to comment on the analogy thus proved to exist between the capillary vessels of vegetables and those of animals—each of them possessing a dilating or absorbing, and a contracting or expulsive power, alike necessary for the circulation of their fluids.

These observations we shall endeavour to maintain by facts, proving that the capillaries of animals, like those of vegetables, attract fluids from the large vessels, and thus materially assist the vis a tergo, and also that this capillary influence, in many instances, is alone adequate to cause the motion of the blood.

A. Capillaries draw blood into their cavities.

g. Dr Philip and Dr Hastings* state, that in live dissections, the circulation in the smaller vessels will continue for several minutes after excision of the heart. In the mesentery, Dr Philip has observed that the circulation proceeded with regularity for half an hour after the removal of the heart†. If contraction were the only vital action, the blood would be driven out of the small vessels; and as no vis a tergo existed from the heart, their dilatation could not occur, but the vessels would remain collapsed and the part bloodless, as in syncope and local asphyxia. Professor Horner also informs us, that during live dissections, the capillary vessels remain distended, after the heart has ceased to act, and so long as any blood remains in the arteries,—the larger vessels becoming first empty, then the smaller ones, and finally the capillary tissue.

These phenomena cannot depend on mere vital contraction of the vessels: for such contraction would occur more readily in the capillary system, the irritability of which is greater than that of the arteries; and this contraction would not, as shown in Proposition I., be overcome by the vis a tergo from the contraction of the arteries. Hence, the influence from the heart being removed and the capillaries contracting, the blood would remain in the arteries, on the supposition that contractility were the only vital property. But the arteries are found empty before the circulation has entirely ceased in the capillaries, and while these last remain turgid. The capillaries hence manifest a dilating power, drawing blood into their cavities, and assisting the arteries in discharging their contents. This being effected, the contractile powers of the capillaries throw the blood eventually into the venous system where it remains. In this way, I think, the emptiness of the arteries after death may be satisfactorily explained.

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* Essay on Bronchitis, p. 51.

† Essay on Vital Functions, 2d edition.

h. The above explanation is confirmed by attending to those few cases in which the arterial system remains distended: as when death has been suddenly caused by some powerful impression, as by lightning, a blow on the stomach, &c. Here the whole of the circulatory apparatus appears to be paralysed, and the blood is generally found fluid. No vascular action continuing after the cessation of the vis a tergo from the heart, the blood remains at rest.

i. Stimuli cause an afflux of fluids to the part irritated. In Proposition II. *g*, *h*, *i*, *k*, various examples were detailed to prove the active enlargement of the vessels under irritations: in all such cases, fluids flow in increased quantities to the part. Whence this afflux? It is not owing to a vis a tergo from the heart and arteries; for this remains as before, and the capillaries are not debilitated (Proposition II. *g*, *h*, *&c.*). And even if they were debilitated, the part would not be distended by the vis a tergo (Proposition I.), and of course no increased flow could occur. The fluids are evidently attracted to the part irritated, and, as dilatation must precede their entrance, I infer that such dilatation, excited by stimuli, is the immediate cause of all such local determinations of blood. This view is confirmed by the fact, that the afflux is proportioned to the irritability and vascularity of the part and to the severity of the stimulus. They increase and decrease *pari passu.* The contraction of the heart, acting upon a single column of blood, can have no effect in increasing or diminishing the flow of blood to any tissue. Such local changes depend therefore on local causes, and chiefly on the varying degree of excitement in each part.

k. The influence of the capillaries in drawing blood from the larger vessels is often exemplified in the operation of sinapisms, blisters, and other rubefacients in prostrated states of the system,—where the surface is cold and pale or livid, where the pulse can hardly be felt, and the phenomena of life are almost extinguished. What is the modus operandi of such remedies in exciting reaction? Do they first increase the vis a tergo, the actions of the heart and arteries, that the capillaries may be distended? or do they not primarily excite the torpid capillaries, whence dilatation and afflux of fluids, and afterwards, secondarily and sympathetically, the arteries and heart? The last view seems correct, because such local irritations are often excited, when the arteries and heart do not sympathize; and in all cases, the capillary excitement precedes the general excitation.

l. The suction power of the capillaries is exemplified in some cases of nævi materni and in aneurisms from anastomosis. Dr SMITH, in his essay (p. 251), relates the case of a lady who had a vascular tumour on the neck, somewhat pendulous. A hemorrhage often occurred, sometimes guttatim, occasionally in a stream, and in many instances, was excited by mental agitation. The excitement of mind, prior to its extirpation, brought on a bleeding; but when the tumour was removed, not three drops of blood were effused from the incised wound. If the original hemorrhage were owing to a vis a tergo, the bleeding should have been augmented, for larger vessels were opened, and the action of the heart and arteries was undiminished; but in this case "the vascular tumour seemed to operate like a pump attracting the blood from the surrounding parts, and discharging it from the system." All surgeons notice the danger of cutting into such tumours, and yet, as Dr SMITH observes, no one hesitates to extirpate them by making incisions into the surrounding parts.

m. Acute dropsies and hemorrhages prove not only the dilating power of the capillaries (Proposition II. *q*), but their suction influence; for although such effusions usually occur when the general circulation is excited, yet they are often met with, when the vis a tergo is at or even below the healthy standard. In these cases, the discharge being augmented, and the vis a tergo lessened, the supply must be kept up by the absorbing or suction influence of the irritated vessels.

This opinion is confirmed by the facts adduced in Proposition I. showing that dilatation was not the result of a vis a tergo, and also by an experiment of BICHAT, who endeavoured, but unsuccessfully, to produce effusion by injecting the vessels of a living animal. The vessels will only enlarge when appropriately stimulated.

n. The reunion of parts which had been entirely separated from the body, a fact now well attested, shows that capillaries absorb fluids from supplying vessels. An amputated finger, when pale, bloodless, and apparently dead, reapplied to the stump, will have its circulation restored, certainly not by a vis a tergo, nor by a contraction of its own vessels, but by a suction power in these vessels; so that the fluids effused by the stump are absorbed for the nourishment of the injured member. Mr HUNTER's well known experiments of transplanting teeth, spurs of cocks, &c., succeeded on the same principle.

o. The rapid enlargement of the capillary and smaller arteries after the main artery of a limb has been tied was alluded to in Proposition II. *l.* The afflux of fluids to the cold and bloodless limb is no less evident than the dilatation of the vessels, and as the vis a tergo is diminished, while the redness and temperature are augmented, we must infer that the blood was moved by some other power than that derived from the heart and arteries; that the excited capillaries absorbed blood from the superior portions of the limb.

Dr N. R. SMITH, of the University of Maryland, records an interesting example of this kind*, in which a ligature was applied to the anterior tibial artery for a wound of this vessel. The hemorrhage from the wound, and the pulsation of the artery on the top of the foot, ceased on drawing the ligature; but after some hours, the bleeding returned from the inferior orifice. The artery on the instep now pulsated vigorously, the beat was stronger and more full in the wounded limb than in the other, and the blood on experiment was found flowing in a retrograde direction. The circulation here was more rapid by anastomosing channels than when the blood pursued its direct course.

p. Professor SMITH was called upon to amputate the leg below the knee for a dry gangrene of the foot and ancle. The great arteries were found ossified, being mere tubes of bone. "Though all pressure was removed from the artery above, and no other means were used to suppress the hemorrhage, yet not half a table spoonful of blood followed the operation. At the same time, the action of the heart, and of the arteries at the wrist, was vigorous."—p. 205. As no coagula existed in the arteries, and as the anastomosing vessels were not enlarged while the parts below had been supplied with some blood, it results that blood must have passed through these ossified tubes before the operation, and that its motion was not owing to the vis a tergo, as the open ossified tubes did not bleed when divided. Hence "the active power lost by the division of the arteries, resided in the capillary system of blood vessels of the foot and leg."

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* Philadelphia Monthly Journal, vol. ii. p. 18.

Similar remarks may be predicated of all large arteries passing through bony canals. In the sturgeon, a large portion of the principal artery passes through cartilage or bone. Hence we infer that the circulation of blood in the large vessels is dependent on a suction power in the capillaries as well as on the vis a tergo. Other facts confirm this important doctrine.

q. The circulation of blood in the placenta proves the suction power of the capillaries*. The impetus the blood possesses in the umbilical arteries is attributed solely to the vis a tergo; but it is well known that after the detachment of the placenta from the uterus at the birth of a living child, the pulsation in the cord ceases—first near the placenta, and finally at the umbilicus of the infant, and that a section of the cord in such cases is not followed by hemorrhage. Why does the flow of blood cease in these tubes? The vis a tergo is as powerful after birth as before, and yet no hemorrhage is caused, when this impetus is operative on blood in tubes free from obstruction. Dr SMITH says it certainly will not be attributed to cold, for the circulation continues in every part of the infant; nor to exhaustion from hemorrhage, for the fœtus loses no blood, as its circulation is independent of the mother. Neither is it owing to the lungs diverting blood from the placenta; for although a greater portion of blood is carried to the lungs after than before birth, yet this would not account for the cessation of the circulation in vessels so large as the umbilical arteries. Animals die from hemorrhage when divided arteries of their size remain unsecured. The solution therefore on common principles is not evident. From those now advocated, it results, that as the organic vital actions of the placenta depend on the parent's system; when this body is detached from the uterus, the functions of the capillary tissue cease. The small vessels by their suction power no longer assist the vis a tergo in carrying the blood through the umbilical arteries, and this last being by itself inadequate to this object, the circulation declines first in the placenta and finally at the umbilicus.

r. Surgeons have occasionally operated for the cure of aneurism, by tying the artery *beyond* the tumour. On the common doctrines of the circulation, the obstruction to the artery would increase the impetus of blood, and the swelling would thus be aggravated; but experiment proves that the tumour gradually ceases to pulsate and diminishes in size*. The vis a tergo, deprived of the assistance of the suction power of the capillaries, is not adequate for the enlargement of an aneurismal tumour.

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* Professor SMITH, Phil. Monthly Journ. vol i. p. 252.

s. As already noticed (Prop. II, *y*), the brachial artery enlarges in varicose aneurism at the bend of the arm. In this case, as part of the arterial blood is lost by the vein, the wants of the capillaries are not sufficiently satisfied; hence a greater quantity of blood is required in the injured than in the sound limb. The vis a tergo operating equally before and after the injury cannot satisfy this increased demand, unless assisted by some local attractive power. The suffering capillaries therefore draw the blood to them; the artery is sympathetically excited, enlarges, and thus admits the necessary supply to the injured member. The experiment of PARRY (Proposition II. *z*), showing the enlargement of one carotid when the other was tied, is a similar fact, alike evincing the attractive influence of capillaries and the consequent sympathetic enlargement of the supplying vessels, until they satisfy their demands. Vide also Proposition II. *s*, *t*, *u*, *v*.

t. The hemorrhage after amputations is trifling when compared to that arising from the division of the same arteries under other circumstances†. Very often the surgeon has merely to secure the main artery, and occasionally, no ligature is required. Professor SMITH has three times amputated below the knee without employing a single ligature; there was no faintness and no unusual diminution of the vis a tergo in these cases: and a German surgeon, Dr L. KOCH, has lately affirmed‡ that neither he nor his father have ever applied ligatures, even to the femoral artery, after numerous amputations in a practice of twenty years. On the contrary, every one is acquainted with the difficulties and dangers attending the division of even small arteries in a healthy limb, giving rise to extensive ecchymoses, to false diffused and circumscribed aneurisms, and often baffling the best efforts of the most scientific surgeon. Whence this difference, unless that in the one case, the influence of the capillaries has been removed by the knife of the amputator, while in the other, blood i continually drawn into the limb by the capillaries, suffering from the loss of blood. (Vide sections *o* and *s*).

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* Vide North Amer. Med. and Surg. Journ. vol. i. p. 388, and vol. v. p. 227, for a notice of these operations.

† Professor SMITH, p. 203, Op. cit.

‡ Jour. des Progrès—and Am. Jour. of the Med. Sci. vol. i. p. 462.

u. Certain states of the system appear to me to confirm the opinions here advocated, and to demonstrate their practical importance. Every experienced physician has observed, especially among intemperate patients, cases, in which the vital energies of the capillaries are prostrated; the surface being cold, clammy, and livid or pale, while the heart and large arteries are in vigorous action,—the pulse being full and tense. This condition is very dangerous; for the heart and arteries under these circumstances soon cease to propel the blood, and the patient perishes, unless the capillaries recover their powers.

The reverse is also true, that patients with an active capillary circulation, and yet with a weak action of the heart and arteries, are comparatively safe. The blood continues its motion with facility when receiving but a trifling impetus from the heart, so long as the capillaries are active; but when these are prostrated, a powerful vis a tergo from the heart and arteries fails, not only in dilating the peripheral system of vessels, but in sustaining the circulation of the vital fluid. Congestion of the large viscera and death soon succeed.

B. The capillaries, thus endowed with expansibility as well as contractility, are often alone adequate for the circulation of the blood.

v. In the invertebral animals, there is no heart; the fluids taken up by the absorbents being propelled by powers analogous to those possessed by vegetables. Proposition III. *a*, *b*, *&c*.

w. In fishes, the heart is single, and is devoted to the lesser circulation in the branchiæ or gills. The general or systemic circulation receives no other assistance from this organ than what may be communicated through the capillaries in the branchiæ. In some fishes, as the sturgeon, the main artery passes through cartilage or bone, thus depriving the blood of the influence of its contractions.

x. The blood in the vena portarum cannot be propelled by the heart and arteries, except through the capillaries of the spleen, pancreas, stomach, and intestines. Its motion, therefore, like that of the sap in vegetables, must chiefly depend on the vessels, forming its roots and branches.

y. The same observation is true, *a fortiori*, as regards the blood in the umbilical vein.

z. We find also from the experiments of HALLER, SPALLANZANI, BOYER, PHILIP, THOMSON, HASTINGS, and GENDRIN, that the blood in capillaries will often flow to an irritated point, and in cases of obstruction, be diverted to other parts, in opposition to the vis a tergo from the heart and arteries.

From the facts and arguments which have been presented for the support of the present Proposition, it results that the contraction of the heart and arteries is not of itself adequate to the propulsion of the blood ; and that capillaries, possessing a dilating power, draw blood into their cavities, and thus assist the impetus derived from the heart. The combined influences, derived from the contraction of the heart, arteries, and capillaries, impel the blood forward into the veins, where further assistance is rendered by the contractile power of these vessels, and when near the heart, by the suction influence of this organ* and of the thorax†. All parts of the circulatory apparatus thus assist each other, instead of furnishing obstacles to the circulation of the blood, as maintained in the present systems of physiology.

That none of the arguments drawn from the state of the parts in inflammation may be invalidated, I shall add a fourth Proposition respecting the circulation of blood in inflamed parts.

PROPOSITION IV.

During the activity of inflammation, an increased quantity of blood flows through the affected part.

It is contended by PHILIP, HASTINGS, and most English writers, that the blood moves slowly through inflamed parts, and that it becomes nearly or altogether stagnant. A large proportion also of the continental pathologists, Dr PRUS not excepted‡, while maintaining the vital erection of the inflamed tissue and the activity of the capillaries, acknowledge the stasis of blood during the inflammatory process. The capillaries, they say, are ineffectually labouring to transmit the blood, and their coats, like the sides of an obstructed hose, continually but unsuccessfully react on the contained fluid*. There is, however, no evidence of obstruction during inflammation. This is a conceded point among modern pathologists, who also admit that obstruction would not give rise to the phenomena of inflammation. Prop. I. *f.*

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* Proposition II. *aa*, *bb*, *&c.*

† HUNTER, CARSON, BARRY, &c.

‡ Op. cit. p. 62.

In support of the opinion that the circulation is more active in inflamed tissues, the following facts may be appealed to with much confidence.

a. The colour of an inflamed surface is brighter, resembling that of arterial blood. Whenever the motion of blood is retarded, the part becomes of a dark livid or venous colour; as observed when the circulation is impeded by ligatures to veins, by bandages to the extremities, or by a dependent position. The dark colour is also noticed in parts exposed to cold; in typhous and malignant fevers; in intemperate patients; in gangrenous surfaces; and in patients in articulo mortis.

b. The temperature of the skin when inflamed is increased, as proved by the experiments of Mr HUNTER and Mr A. COOPER. This is indicative of activity in the circulation, as a cold surface denotes its depression.

c. Pressure made on the capillaries empties them of blood, and when this pressure is removed, the blood returns with great rapidity during inflammation, but very slowly in torpid states of the circulation.

d. Incisions made into inflamed parts are followed by a free hemorrhage, which can with difficulty be restrained. The blood is emitted not only in greater quantity, but with greater impetus and of a brighter colour.

e. Large quantities of blood are conveyed to an inflamed part, as manifested by the fulness, tension, and pulsation of the supplying arteries, and by the force with which they throw out blood when divided. What becomes of the blood thus abundantly conveyed? If this fluid were stagnant in the capillaries, rupture or gangrene would speedily ensue from the impetus and quantity of blood thus driven into the engorged capillaries.

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* The writer of the article Circulation, in the Dict. Abrégé des Sci. Med., admits an increased circulation in inflammation, unless perhaps in chronic inflammation. GENDRIN, in his review of THOMSON, Jour. Géneral, Aug. 1827, p. 319, says, that vessels in inflammation become more permeable, and that the circulation is at first greatly accelerated, but finally becomes arrested.

f. The veins coming from an inflamed part are enlarged, and this in greater proportion, says Mr HOME, than the supplying arteries. Their size increases also in proportion to the intensity of the inflammation until gangrene occurs. Hence I infer, that more blood comes from an inflamed part.

g. That the blood carried to an inflamed part is not stagnant, or even retarded in the supplying arteries, in the affected capillaries, or in the receiving veins, is further demonstrated by the change of colour, and the increase of pain and swelling produced, when the circulation is impeded by a dependent posture, or by ligatures or bandages pressing on the veins; and by the force with which blood is emitted from a receiving vein when punctured, by the bright colour of the blood thus effused, and by the pulsatory motion which it often exhibits under these circumstances*.

h. The abundant secretions of serum, lymph, and of pus, so frequently observed, could not be formed without a free supply of the pabulum vitæ. So also, the reunion of divided parts and the generation of new substances, demand a greater supply of blood as well as a more active exercise of the vital properties than simple nutrition.

The phenomena and consequences of inflammation thus evince the close analogy which exists between this morbid state and those various processes of the animal system, in which every one recognizes an increased circulation; such as the renewal of a stag's horns, the growth of tumours, the erection of the penis and nipple, the enlargement of the uterus, the distention of the mammæ, and the subsequent secretion of milk, &c. The one class of phenomena manifests as decidedly as the other all the evidences of capillary excitement, and of an increased flow of blood through the affected tissues.

If the object of this essay has been attained, it results that expansibility is a vital property, as well as contractility, and that both are modifications of the irritability of many modern pathologists: that the heart, arteries and capillaries, possess this property of dilating under the influence of their normal stimuli, but do not enlarge from any vis a tergo to which they are subjected; and finally, that they contribute by active expansion and contraction, at regular or irregular intervals, to the ready transmission of that vital fluid which is essential not only to secretion and nutrition, but also to reparation.

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* This has been observed by many practitioners. Vide HUNTER on the Blood, &c. p. 161. 163.

ARTICLE II.—*Observations on Hereditary Hemorrhage.* *By* REYNELL COATES, M.D.

There are several families, in various parts of the United States, affected with a singular predisposition to hemorrhage from very trivial injuries. Dr OTTO, of this city, has written a paper, which will be found in the sixth volume of the New-York Medical Repository, giving an account of this idiosyncracy, as displayed in the descendants of a Mrs Smith, who settled in the vicinity of Portsmouth, N. H. about eighty years previous to the date of his communication. Nearly all the male progeny of this lady were subject to dangerous and often fatal hemorrhage from the slightest wounds or scratches; nor could the most enlightened professional skill suggest any effective remedy, until it was accidentally discovered that the *sulphate of soda* exercised a control over these cases, as unaccountable as the affection itself.

The females of the family never exhibited this predisposition, although they transmitted it to their male issue. Those who were affected were robust men of a choleric disposition, and many of their neighbours supposed that they could distinguish the "*bleeders*," as they were called, by certain peculiarities of personal appearance.

Another paper, by the same gentleman, published in Coxe's Medical Museum, vol. i. contains the particulars of the fate of four sons of Mr Benjamin Binny in Maryland; the first of whom lost his life by a hemorrhage, occasioned by the "kick of a horse over the eyebrow;" the second, from "the fall of a brick on the forefinger while at play;" the third, from "a cut over the eye from the swing of a gate as he was passing through;" and the fourth, who was subject to frequent attacks of epistaxis, fell a victim, in early life, to dropsy, consequent to the loss of blood. Physicians attended each of them, but to no purpose. The females of this family, also, were exempt from the affection.

Dr Otto mentions that Dr Rush was twice consulted in cases of the same hereditary tendency. Di Physick recently informed me, that he had attended a patient, about thirty years ago, in whom a slight cut upon the knee produced a very obstinate hemorrhage of a similar character; and I have lately been informed of two other families, one in Pennsylvania, and the other in Georgia, in which this idiosyncracy is strongly marked.

These instances are sufficiently numerous to render the history of the disease highly interesting to those practitioners who reside in the neighbourhoods where it prevails, and to such I hope the following case, in which I was recently consulted, will prove worthy of perusal. If the details of treatment should be considered unnecessarily minute, I must rest my apology upon the desire to avoid that careless and generalizing style of description, which renders too many reports of this character altogether useless to those, who appeal directly to facts, and who are unwilling to place implicit confidence in the deductions of others.

R. W. a young gentleman from Delaware county, Pennsylvania, a member of the medical class, was the first of his family to display the idiosyncracy, which is the subject of the present paper. His frame is vigorous, and he has every appearance of health.

His sister has never exhibited any uncommon disposition to hemorrhage, but, among her children, the following cases have occurred. A child, aged about eighteen months, fell and ruptured the frœnum of the upper lip. Two of the most respectable practitioners of the neighbourhood exerted themselves to save the patient, but in vain: he bled to death.

Another child, aged about fifteen months, met with the same accident. Three physicians were consulted; but the hemorrhage continued until the third day after the injury, when the child was taken to Dr Gideon Humphrey, in a state of extreme exhaustion. This gentleman succeeded in arresting the bleeding by means of the actual cautery.

The same child, when nearly thirteen years of age, received a cut upon the forehead by a blow from a stone, and was attended by two practitioners who employed the actual cautery freely, but without effect: the patient fell a victim to the hemorrhage.

These cases are mentioned by Dr Humphrey in the Medical Review for September 1824. The slight discrepancy between the statements contained in that paper and the account here given, is owing to the fact, that the former was published from rough notes, taken long after the event occurred, and not intended to meet the public eye; while the latter has been corrected by referring anew, to Dr H., and also to the family affected.

Dr H. has since narrated to me two other cases in the same family. In the first case, which occurred in the brother of the children already mentioned, *a slight contusion* of the scrotum produced an enormous extravasation in the cellular tissue; and in the second, a wound of the thumb was followed by bleeding for several days. The *shower bath* suddenly arrested the hemorrhage.

R. W., the gentlemen above mentioned, received a cut from a penknife on the forefinger, when about eleven years of age. The wound continued to bleed without intermission; and on the fifth day after the accident Dr HUMPHREY was consulted. He applied a compress and bandage, "sufficiently tight to stop the discharge;" but on the following day he found the finger considerably inflamed, and the hand and forearm distended with extravasated blood. On removing the compress, the blood again flowed from the wound. The Doctor then applied remedies to combat the local inflammation, and continued them for a few days. The bleeding, however, did not cease, and the patient being in an extremely exhausted condition, the actual cautery was applied with complete success.

In the autumn of the same year, Mr W. had a molar tooth extracted. Blood continued to ooze from the gum for many days, nor was the hemorrhage checked, until the actual cautery was liberally employed as a last resort.

I shall now proceed to give the details of another similar attack, occurring to the same gentleman, when 24 years of age, as extracted from my case-book.

22d December 1827, 5 o'clock, P. M. I was requested by Mr (now Dr) SPACKMAN to visit Mr W. who had a tooth extracted by a dentist the day before. The patient and his friends informed me, that the hemorrhage following the operation was so profuse and of so long continuance, that the dentist, at the request of Mr W., applied the actual cautery, but without success. Mr SPACKMAN took charge of the case at 10 o'clock, A. M.; but being unable to check the discharge, he thought it advisable to call in additional aid.

The patient had lost at this time about half a gallon of blood. The measures resorted to by Mr S. were as follows:

1st. The sulphate of copper was applied, in the solid form, to the bottom of the socket without effect.

2d. A piece of cork, adapted to the form of the cavity, and wet with a strong solution of the same salt, was employed as a compress, with no better success.

3d. The cavity was plugged with lint, but without any benefit.

4th. A saturated solution of the sulphate of alumina was employed as a gargle; but this did not arrest the bleeding.

5th. A piece of sponge, imbued with this solution, was employed as a compress; a piece of cork was applied over it, and pressure was made by means of BARTON's jaw bandage. This apparatus arrested the hemorrhage, but it returned in four hours.

6th. Dry sponge covered with powdered sulphate of alumina was applied, and secured in the same manner. This application lessened the amount of discharge considerably, but did not entirely check it.

At the time of my visit I found Mr W. weak and pale, with a pulse small and frequent, but quite mild. His stomach was irritable. The bleeding at this time being slight, I did not think proper to advise the removal of the dressings; but recommended that the patient should be kept cool and quiet, in the hope that the hemorrhage might cease, without obliging us to resort to more severe measures.

23d December. I was called to Mr W. in haste, at 10 o'clock, A. M. and found him bleeding profusely. The dressings were removed, and in order finally to test the effect of pressure, applied in the most careful manner, I proceeded to plug the cavity of the socket with dry lint, firmly impacted by means of a bent wire. Over this was placed a firm compress of lint, surmounted by a piece of cork, upon which the inferior jaw was strongly closed, by means of BARTON's bandage. This apparatus totally suppressed the discharge until after daylight, when it returned.

At 10 o'clock, A. M. the hemorrhage was very considerable. I reapplied the compresses, moistened with a saturated solution of sulphate of copper. The bleeding was rather less profuse for half an hour; but after this time it became even more copious than I had yet seen it.

At 11 o'clock, Mr SPACKMAN being present, we made a very careful examination of the wound, and discovered that no blood escaped from the socket of the tooth, but, on the contrary, that the discharge took place from one angle of the incision made by the gum lancet, just within the tooth next adjoining, which was the first of the molares. The whole cheek and gum were much swelled and inflamed, in consequence of the pressure which had been employed; and there was some febrile action. The actual cautery was now applied to the bleeding surface, and the hemorrhage became instantly arrested. The patient was ordered to take a purge of the sulphate of magnesia immediately; to be followed by the acetate of lead and the nitrous powder*, alternately exhibited.

At the request of the patient, Dr GIDEON HUMPHREY was called in consultation.

At 3 o'clock P. M., Dr H. met us. The hemorrhage was very slight and no change of measures was proposed. Dr H. stated that, in all the cases occurring in this family, pressure invariably occasioned severe inflammation, without producing any benefit.

At 11 P. M. I found the hemorrhage as profuse as ever. I re-applied the cautery freely and checked the discharge.

26th December. I did not see the patient on the 24th or 25th, concluding that as I had not been sent for, the case had terminated favourably. Mr SPACKMAN, however, informed me, that the hemorrhage had recurred on the morning of the 26th, and continued without intermission, *notwithstanding the application of almost every styptic, which suggested itself to Mr W. and his attendants.* Most of the remedies appeared to lessen the flow for a short interval, but their ulterior effect was rather to increase it.

At 10 o'clock this morning I called, and found the patient absent. Weak as he was, he walked to Dr H.'s office, where the cautery was again applied very freely to the inner flap, the bleeding being confined to that part only. The discharge was again checked, but returned as violently as ever on his arrival at his own residence. The cautery was frequently reapplied by myself and others during the day and night, but without permanent advantage.

27th December. The consultation met at 10 o'clock, A. M. The local inflammation had in a great degree subsided. Mr SPACKMAN stated to us, that, at 1 A. M. Mr W. had been attacked with vomiting, purging, and considerable pain about the colon. With a view to relieve these symptoms, Mr S. exhibited calomel, grs. iii. cum pulv. opii. gr. i. and directed tinct. opii, gtt. xxx. quâque tertiâ horâ sumend.

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* The nitrous powder contains nitr. potass. grs. x. with tart. antim. gr. 1-6th. It is a formula employed in the practice of the Pennsylvania Hospital.

The patient was now free from fever, and the purging had ceased, but the stomach continued irritable. This symptom was relieved by a spice plaster to the epigastrium, and a Seidlitz powder, which operated freely. The hemorrhage was very profuse, and the patient extremely exhausted. Tinct. opii continued.

It was resolved to day to make a final conclusive trial of the cautery, under the idea that some portion of the flap might have escaped contact with the iron. I, therefore, proceeded to destroy, by this means, not only the wounded surface, but the whole thickness of the gum, and surface of the socket. The blood continued to ooze rapidly through the substance of the slough, and about an hour after the application of the iron, I *distinctly saw the hemorrhage proceeding from the surface of the uninjured* gum, at some distance from the sloughs. This exudation continued for a short time only.

At the request of the friends of the patient, Dr J. R. BARTON was invited to join us in consultation.

At 2 P. M., the consultation met. Hemorrhage considerable. The blood contained very little fibrin. The patient was exceedingly pale, and his pulse very weak, but perfectly mild. The amount of discharge since the commencement of the case certainly exceeds two gallons. It was agreed that a nourishing diet had become indispensable. He was, therefore, ordered to take plentifully of soups and jellies. Eggs were also allowed him. Port wine was directed to be given pretty freely, and the spirit of turpentine was prescribed, fifteen drops to be taken every two hours.

At 6 P. M. I called. Mr W. had taken about half a pint of Port wine. The spirit of turpentine was immediately rejected from the stomach. The patient had considerable fever, with some stupor and pain in the head. The Port wine and turpentine were therefore omitted, and gruels and beaten eggs, directed to be taken freely: mustard plasters were applied to the feet. During the following night, the hemorrhage declined, *and ceased entirely for an hour after a warm breakfast.*

28th December. At 10 A. M. the consultation met; Dr HUMPHREY absent. The patient was free from fever, and the bleeding not very profuse. The exhibition of Port wine was renewed, and HUXHAM's tincture of bark was prescribed.

12 P. M. Found considerable fever, the pulse beating 122 strokes in a minute, and the hemorrhage profuse. Applied the muriated tincture of iron, and at the earnest desire of the friends, permitted the application of a popular remedy, the oil obtained from burnt linen. Both these articles lessened the discharge for a short time, but ultimately produced an exacerbation. The compound tincture of bark being invariably rejected, its exhibition was discontinued.

29th December. At 11 A. M. the consultation met. Mr W. informed us that the hemorrhage had not abated, until he had taken breakfast, after which *it had stopped for half an hour.* At the time of our visit, it was considerable. The fever had somewhat abated; pulse 115. Notwithstanding the free purging which followed the exhibition of calomel, two days before, a strong mercurial odour was perceptible in the patient's breath. Ordered sulph. quiniæ, gr. i. q. tertiâ. hor. sum., et tinct. ferri, gtt. v. q. hor. sum. *Tinct. opii continued.*

At the desire of the friends of the patient, Dr PHYSICK was requested to join us in consultation.

At 4 P. M. Dr P. and myself met, the other gentlemen being absent. One important indication in the treatment of hemorrhages from the mouth in general was suggested by Dr P. He stated that he had met with instances of extreme exhaustion from bleeding, consequent to operations in that cavity, and which continued after all the remedies, usually resorted to, had been employed to no purpose. These discharges he supposed might be kept up by the suction, unavoidably attendant upon the effort to swallow, and were to be arrested by keeping the mouth permanently open.

In order to answer this indication as completely as possible in Mr W.'s case, a plug of wood was introduced between the molar teeth, and he was ordered to receive nourishment through the stomach tube. He was also confined to a sitting posture; and in order to ascertain whether the coagulation of the blood upon its exit would not prove serviceable, when aided by the means just mentioned, it was directed that a slight pledgit of common spunk should be kept in contact with the bleeding surface.

The introduction of the stomach tube was strongly resisted by the patient, under the impression that some injury might be sustained by the œsophagus, in which case a second hemorrhage, less accessible and consequently more unmanageable, might result.

At 1 P. M. the bleeding continued unchecked, notwithstanding the introduction of the plug, which the patient was compelled to remove in about an hour, in consequence of the nausea occasioned by its presence. He took one egg beaten up with sugar and water, and this was his only sustenance for twenty-four hours.

At 11 P. M. I reapplied the plug, but the extreme irritability of the fauces rendered its presence intolerable, and Mr W. removed it at 1 o'clock, A. M. He then continued to hold spirit of turpentine in his mouth for some hours, at his own suggestion; but this increased the hemorrhage very considerably.

Dec. 30th. At 10 A. M. we met. We were informed that the patient had taken plentifully of warm tea for breakfast, after which the hemorrhage *was much diminished for some time.* The mercurial odour could no longer be detected in the breath.

At 4 P. M. Mr Spackman having procured some spunk, introduced a wedge shaped plug between the molares of the injured side, surmounted by a pledgit of that article, according to the directions of yesterday. The hemorrhage, although somewhat lessened for a short interval, ultimately became still more violent. I afterwards carefully reapplied the same apparatus, with a similar result.

The patient was soon afterwards attacked with vomiting and purging. Mr Spackman exhibited laudanum freely, and these symptoms were soon relieved. He then reapplied the plug with temporary benefit; but Mr (now Dr) Temple, in whose charge the case was placed for the night, states that the bleeding became very profuse in a short time. At 3 o'clock, A. M. efforts to vomit rendered it necessary to remove the plug. The patient was now extremely weak, having nearly fainted on attempting to rise. After the final removal of all local applications, the hemorrhage ceased in a little while, *and did not again return until* 5 P. M.

Dec. 31st. At 10 A. M. we again met, all the gentlemen being present. The patient had taken plentifully of rich broths, rice pudding, and other nutritious articles, not requiring mastication. This diet was ordered to be continued. He was directed to prevent the entire closure of the teeth, *but not of the lips*, by inserting a small piece of cork between the incisors. The amount of tinct. opii was reduced to gtt. xxx. every four hours. At 5 P. M. the hemorrhage returned, after a slight febrile paroxysm, accompanied by chilness and headach. It continued to flow, gently, until 3 o'clock in the morning.

1st January 1828. In the afternoon of this day there was a slight return of bleeding, which continued two hours, and then finally ceased. The whole amount of blood lost during the progress of the case cannot be estimated at less than three gallons.

It is proper here to remark that Mr W. has received several wounds from the sickle, while engaged upon his father's farm. One of these, which was a severe and deep cut upon the hand, produced no unusual discharge of blood, but several others, of less extent, required the application of the actual cautery. The slight scratches occasionally inflicted by the razor seldom caused any trouble.

Remarks.—Whether the effects of pressure are similar in all cases of this strange idiosyncracy, is a question that cannot be determined from the limited evidence which is at present within my reach; but it is evident that among the relations of Mr W. it is not only unavailing, but even productive of much inflammation, and, in some instances, of extensive ecchymosis. In the case just narrated, it will be perceived, that pressure, in common with many of the styptics employed, exerted some control over the discharge upon its first application; while it eventually produced an exacerbation, by increasing the local irritation.

During the first few days, the patient ate but little, and, as he was at the same time freely purged, his strength rapidly declined; but after a generous course of diet was instituted, the disease became more manageable. It was curious to observe how regularly the hemorrhage intermitted after breakfast, which was his most considerable meal. This effect was probably owing to the concentration of blood about the stomach, during digestion, acting in the manner of a counter-irritant. A plentiful supply of food appeared absolutely necessary, to support the patient under the immense drain to which he was subjected; but the effect of the tonic and stimulant remedies resorted to towards the close of the case, sufficiently prove, that however great the debility may be in such cases, *no benefit is likely to result from any thing which tends materially to increase the activity of the general circulation.*

The actual cautery, so generally successful in former injuries occurring in this family, failed entirely in the present case; and it is worthy of remark, that the temporary benefit resulting from it, became less apparent, in proportion to the severity with which the process was conducted.

The employment of cauterizing irons is so exceedingly unpopular at the present day, that many surgeons have scarcely witnessed it: they are, therefore, generally ignorant of the fact, that the application of the iron at a clear white heat is productive of little pain, and almost no inflammation; but that when the temperature is less considerable, much pain and irritation are immediately produced, followed in many cases by severe inflammation.

An iron, small enough to enter the socket of a tooth, will inevitably lose much of its heat between the fire and the mouth. An additional portion will be dissipated by the moisture of the part before it can act upon the solids; while the steam resulting from the application, together with the radiant heat of the iron, must necessarily stimulate the neighbouring surface, in defiance of every precaution. The evident exacerbation of symptoms produced by the cautery in Mr W.'s case, resulted, no doubt, from all these causes combined, and should caution us against depending too much upon it in such affections.

The phenomenon which presented itself after the final application of the iron, (I allude to the escape of blood from the uninjured gum) clearly demonstrated that a wound of any kind is not in all instances necessary to the production of this disease. One of the cases mentioned in the editorial notes upon the paper of Dr HUMPHREY, goes to establish the same fact, and appears to prove that some affinity exists between this complaint and purpura hemorrhagica.

The case is extracted from a communication by Dr ELSSAESSER, published in HUFELAND's Journal, for Feb. 1824. The patient was constantly subject, from the time of dentition, to numerous subcutaneous extravasations of blood, forming maculæ, from the eighth of an inch to an inch in diameter, which disappeared in five or six days, after undergoing the usual changes of colour, giving place to fresh eruptions in other spots. This patient fell a victim, in childhood, to a hemorrhage caused by a slight wound on the head. His two brothers evinced the same strange idiosyncracy.

It does not appear that any of the cases to which I have alluded have been attended with hemorrhage from considerable arteries; on the contrary, the blood seems to ooze from the capillaries alone. Among the descendants of Mrs Smith, the discharge generally commenced several days after the injury, at a time when the wound was partially healed. In the sickle cut which Mr W. received, I have reason to believe, judging from his own description of the symptoms, that no unusual amount of arterial bleeding occurred; and in Dr Elsaesser's case, death was occasioned by the accidental removal of a scab from a very slight cut, situated on the patient's head.

From these facts, it would appear, that this peculiar flux, even when occasioned by a wound, has nothing in common with simple traumatic hemorrhage. It is, therefore, less surprising, that the ingenious contrivance suggested by Dr Physick, was not productive of the same benefit in this case, which had resulted in those wherein he had previously employed it. The permanent extension of the cheeks and the action of the air, stimulated the salivary glands, and increased the activity of the circulation in the neighbourhood of the injured part. The coagulation of the blood on its exit, by means of the spunk, was of no avail. Indeed, whenever the wound was left undisturbed for any considerable time, the whole surface would be found covered with a dense coagulum, but this did not lessen, in any degree, the rapidity of the discharge.

I would not, however, be understood to say, that the prevention of suction was unimportant in the case of Mr W. On the contrary, I think it highly probable, that the disease proved less obstinate, in consequence of the slight separation of the incisors, which was kept up by the cork, introduced between them. Over this the lips were closed; involuntary suction was prevented, and the salivary glands and fauces were not stimulated to undue action. But the successful termination of this formidable affection is, I think, mainly referrible to *the timely adoption of an invigorating plan of treatment, and the free employment of the tincture of opium.* Indeed I am convinced, that had all stimulant and local treatment been omitted on the 27th of December, the case would have terminated more speedily.

Since the preceding pages were written, I have met with several other notices of hereditary hemorrhage, in which are contained facts interesting in themselves, and illustrative of the observations contained in the present communication.

An essay by Dr John Hay, of Reading, (Mass.) published in the N. E. Journal, vol. ii. p. 221, gives a very full account of this idiosyncracy, as it occurred in Mr Oliver Appleton of Ipswich and his male descendants through many generations. One of this gentleman's relations married a Mrs Smith, of Haverhill, supposed to be the lady alluded to in Dr Otto's first paper on this subject.

As many of the sufferers were members of the medical profession, there is every reason to suppose, that all the resources of the art were properly tested; but it does not appear that any check was opposed to its fatal career, until the trial of the sulphate of soda.

Cases are mentioned of bleeding from almost every species of slight injury; but the most important facts are as follows:

The predisposition does not display itself in every generation, but descends through the grandsons in the female line. "The bleeders" are all of a light complexion, apparently healthy, and of a choleric disposition. The hemorrhage seldom appears for several days after the wound, *never flows per saltum*, and is sometimes accompanied by considerable pain. A wound is not in all instances necessary to the discharge; for death has occurred from epistaxis, and hæmoptisis; and in one instance, two quarts of blood flowed from the integuments of the hand, after a slight bruise which did not abrade the skin. When ligatures have been applied above a wound, to arrest the discharge, it has burst forth again, *from above the ligature*, attended with much pain.

Dr Hay mentions, that, during the discharge, the "bleeders" are very much troubled with costiveness and vomiting. He states that the *sulphate of soda*, given in the dose of an ounce, and repeated daily for several days, generally proves successful; but that when employed "more frequently," it never fails: he mentions, however, but one case in which it was attended with advantage.

Drs Wm and Saml Buel, have published in the *N. Y. Physico-Med. Journal*, vol. i. an account of a similar family predisposition in the descendants of the Rev. Timothy Collins, the first pastor of the town of Litchfield, Conn. His son was the first who displayed it, and it afterwards affected some of the branches of the family in every generation, confining its attacks to the males. One of the fatal cases resulted from the extraction of a tooth. The hemorrhage *never flowed per saltum*, and the blood always coagulated freely. Those who were affected were of sallow complexion, and subject to plethora and spontaneous hemorrhage from the nose and mouth. *No mechanical or styptic treatment was productive of any benefit.* Pressure usually "distended the part above, rendering it livid and intolerably painful; nor could relief be obtained until the discharge was permitted to continue."

Dr **Elssaesser**'s papers* contain several interesting cases; but it is unnecessary to present them to the reader, after what has been already narrated. Two of the individuals affected were sisters; and these, with one other, are the only exceptions that I have met with to the general rule that confines the disease to males. These females were subject to spontaneous hemorrhage in early life.

Dr E. has also communicated a *post mortem examination*, of one of his cases. The body was found "*bleached*" in almost every part. The heart and large vessels discharged a thin and slightly reddish serum. There was a marked deficiency of secretion in the serous cavities. Many glands in the mesentery, and several in front of the lower part of the trachea were enlarged. The cardiac half of the stomach "was uniformly pale red." The upper portions of the small intestine were pale, but, from the commencement of the ilium to the anus, the canal was reddened; the depth of tint increasing as it descended. This portion contained mucus, slightly coloured in the ilium, but mixed with florid blood in the rectum. The case commenced with hemorrhage from a slight wound, and terminated by discharges of blood from the rectum.

Professor **Nasse**, of Bonn, has published an elaborate paper on this subject†. The original I have not seen, and the slight notice of it contained in the N. E. Journal, furnishes no additional information.

I might refer to many other isolated cases of this disease, and also to a few more instances of family predisposition to it‡, but it is time to close this paper, with a few practical remarks.

Hereditary hemorrhage is a disorder so rare, that very few have enjoyed the opportunity of witnessing a sufficient number of cases, to warrant them in adopting general views of its pathology; and, unfortunately, the recorded facts are not so numerous as to supply the deficiency. It must be left for others, at some future time, to detail with accuracy the morbid appearances, and peculiarities of temperament, which characterize the "bleeders." Setting aside all speculation, I shall content myself with offering a few comments upon the preceding cases, and some practical conclusions deduced from their history.

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* **Hufeland**'s Journal, February and September, 1824.

† **Horn**'s Archives, May and June, 1820.

‡ Vide Ed. Med. and Surg. Journal, vol. iv. p. 513, and N. E. Journal, vol. vi. p. 235.

All the evidence before us tends to show, that no great aberration from the customary state of health is necessary to produce this hemorrhage in the "bleeders." In many instances, no wound or obvious lesion of any part is necessary to the attack. The blood vessels appear to be at all times ready to pour out their contents upon the slightest excitement. The family at Ipswich were robust and choleric, characteristics very consistent with an undue degree of plethora and activity of the circulation. The "bleeders" of Litchfield were sallow and less vigorous; but were constantly liable to attacks of plethora and local determinations of blood, which were generally relieved by epistaxis, or other hemorrhages.

The state of the circulation in the case of Mr W. is very peculiar. His cheeks are generally red, and the other parts of his face pale; but his colour constantly varies with the play of his feelings. His pulse ordinarily beats with an active stroke, and there are commonly about eighty-five pulsations in the minute. When he is engaged in earnest conversation, its rapidity is considerably increased; and I have known it to rise from eighty-two to ninety-five, from the exertion of walking across the floor.

In the worst forms of this idiosyncracy, in Dr ELSSAESSER's case for example, irritations, too slight to be discovered, are sufficient to produce extravasation*. Where the disposition is less marked, a local congestion, as in some of the Litchfield cases, or a bruise which does not abrade the skin, as in one of Dr HAY's cases, may suffice; but more commonly a wound is required to call the disease into action.

It is obvious, however, that few of the "bleeders" would arrive at years of discretion, in the midst of so many dangers, if they were at all times equally liable to hemorrhage. It will be remembered that Mr W. once received a severe wound, which was productive of no serious consequences. It is, therefore, a matter of considerable importance to guard those who are subject to this disease against all causes of plethora, and among these, suppression or diminution of the natural secretions, is the most powerful.

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* See a case of bloody sweat in a young girl. By MESAPORITUS, Phil. Trans. Abr. vol. v.

Dr HAY particularly mentions that his cases were affected with great costiveness and irritability of stomach, *during the time of the discharge.* Such a condition of the bowels could hardly fail to increase the liability to hemorrhage in persons so constituted, and also to render it more obstinate when it occurs, by closing the most important *flood-gate* of the circulatory apparatus, if I may be allowed to employ such a simile.

The condition of the hepatic secretion is also of the highest importance upon the same principle.

In the fall of the year 1824, I attended a woman, who had laboured under jaundice for two years. She was attacked by rheumatism, for the relief of which I performed acupuncture in front of the arm, over the middle of the radius. Blood flowed from the orifice formed by the needle, when not restrained by a compress, and continued to do so, until the time of her death, which happened *three* days afterwards.

While the present paper was in preparation, Dr CONDIE had the politeness to furnish me with some notes of a somewhat similar case which occurred in his practice in 1827. The patient O'M. had been affected with jaundice for some time, and every part of his skin was of a deep yellow tint, inclining to green. He had been intemperate for some years. Dr C. was called to see him on the 23d of June, and found him affected with much derangement of the stomach, costiveness, loss of appetite, and other symptoms of hepatic disease, all which, excepting the colour of the skin, subsided considerably by the 5th of August, under the usual treatment. On the 7th he complained of burning and tickling of the gums with a slight discharge of blood from them. The hemorrhage increased, and by the next morning became very considerable, although there was *neither wound nor alteration of structure* in the part. It yielded readily to a combination of opium, ipecacuanha, and acetate of lead. On the 9th, the discharge returned and flowed from the mouth in a stream. It coagulated beneath the tongue, the coagula forming complete casts of that part of the mouth. When the gums were wiped with a sponge, *the blood was seen to start up at every pore* from the whole surface. The opium, ipecacuanha, and acetate of lead were continued, and cold water was held constantly in the mouth; and when the patient became extremely exhausted, wine and water were administered. By these means, the hemorrhage was checked, but a severe diarrhœa having supervened, death took place on the 10th. The examination of the body was not permitted.

Deductions.

Depletion seems to be imperiously demanded in the first onset of this disease, whenever there exist any marks of general plethora, but with regard to the choice of the means considerable caution is required.

Venesection is hardly admissible; for cases are not wanting, in which the incision made by the lancet has formed a new centre for the hemorrhagic action*.

Purgatives are decidedly proper, even in the last stages of the complaint where there is any tendency to costiveness; for, by such means, we establish a natural flow of secretions, the suppression of which cannot fail to be injurious.

Calomel may be indicated, where there is deficient or morbid action of the liver; but in employing this article we should be careful to recollect, that ptyalism is very readily induced in patients who have lost a large amount of blood; and such an accident could not fail to induce unpleasant consequences in this disease.

Sulphate of soda, which is said to have had a specific effect in hereditary hemorrhage, certainly deserves especial attention, as the only successful remedial measure heretofore communicated to the public; but I should be more willing to place its effect in the *Ipswich cases* to the credit of its purging properties, than to any peculiar power that it can possess.

Tonic Remedies are indispensable at the close of the disease, when extreme exhaustion is induced by the discharge; but those only which appertain to the class of dietetics are unexceptionable.

Direct Stimulants are altogether inadmissible.

Narcotics are strongly indicated, as exercising a powerful influence over irritation; and in those cases occasioned by a wound, they are all important, in consequence of the inefficacy of local treatment. They should be administered liberally.

Local Measures are generally unavailing, with one only exception.

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* See the case already cited in Phil. Trans. vol. v.

Astringents are invariably followed by an ultimate increase of the flux, though they may lessen it for a short period.

Pressure is productive of irritation and inflammation without suppressing the discharge, which, when denied an outlet, will produce either an internal extravasation, or a new hemorrhage from some other part of the body.

Cold was thought to produce some benefit in Dr CONDIE's case; but it was not applied alone, and the remedy was fully tested in the case of Mr W., but without advantage*.

The actual cautery has succeeded in many of the lighter cases; but it should never be persevered in, after it has been once properly applied without advantage.

Potential cautery has been employed in one mild case only, and in this case it was unsuccessful. From what I have seen of its effects in traumatic hemorrhage from small vessels, I should be inclined to view it favourably.

These deductions are not offered to the public as established facts. The ground is in great degree new, and the amount of experience small. My remarks are thrown out in the hope that some advantage may result from placing what is known more immediately before the profession, and from the proofs here given of the inefficacy of the usual plans of treatment. They must be subjected to the test of future observation.

ARTICLE III.—*Case of Epilepsy, successfully treated. By* JOHN C. OTTO, M. D. *One of the Physicians to the Pennsylvania Hospital.*

[Read before the Philadelphia College of Physicians, April 1, 1828.]

I was requested, in November 1821, to visit E. W., aged thirteen years, apparently of a good constitution, stout and of a healthy aspect. His mother stated that he had been subject to convulsions a little more than a year. During the first month of the time, he had four or five, and they have been growing rather more frequent and violent ever since; nor does she know any cause to which to ascribe them. It was observable, from the beginning, that he would have several in pretty rapid succession, and then there would be a respite for a time. For the last three months, from the change to the full of the moon, he has had at least one a day on an average, and, from the full to the change, he has had none. Previous to an attack, his spirits are often unusually elevated for several hours; but he has no immediate warning of its occurrence. Any thing that vexes him much, or highly excites his mind, has frequently occasioned a convulsion. Sometimes even the slightest rebuke, or a refusal to indulge him in his capricious desires has brought it on, which has induced his parents to gratify him in every wish within their power, and frequently beyond what their limited means would justify. When affected, he falls down suddenly and is convulsed, and occasionally froths at the mouth, and his countenance becomes red and distorted: it sometimes lasts but five minutes, and at others half an hour. When prolonged, the convulsions cease for a minute or two and are then renewed, and are always followed by languor and head-ach. From the beginning, not merely his memory, but the other faculties of his mind appeared to suffer and to be gradually impaired. At first he took, under the direction of a physician, three or four purges, and then small quantities of calomel until his mouth became slightly sore, and a gentle salivation was kept up for a short time. He seemed a little better for this treatment, and it entirely freed him from a pain in the right side in the region of the liver. He took no other medicine, except what a person who called himself an Indian Doctor gave him, which consisted of about a tea-cupful of a root, put into three half pints of fourth proof rye whiskey: of this he took a tea-spoonful night and morning, and a table-spoonful whenever the family could perceive the convulsions were coming on. He became gradually worse during this treatment, his mind rapidly verging to fatuity, and after taking a large dose of the medicine, the symptoms of temporary mania were occasionally manifested. In one instance, he pursued his father with an axe with an intent to injure him, and it was necessary to tie or confine him several times. Upon inquiry, I have learned that his paternal grandmother and her sister have occasionally been deranged, and that their brother has been so for forty years. Since last spring, he has passed about a dozen worms of the lumbricus species.

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* Ice was applied to the gums during one night, but, unfortunately, no note of the fact was preserved.

I ordered him some anthelmintic medicine, for worms occasionally aggravate and sometimes produce convulsions ; and when a person is affected with epilepsy, and I have the least suspicion that they exist in the alimentary canal, I am in the habit of giving medicine for their expulsion, preparatory to a different treatment of this complaint. He was likewise to take three tea-spoonfuls of a mixture, consisting of equal parts in bulk of pulverized sage, ginger, and unbruised mustard seed ; or by weight, according to the following formula :

℞.—Pulv. salviæ, gr. xvi.
Pulv. zingiberis, gr. xxx.
Sem. sinapi, gr. L.—M.

This quantity to be given, in water, syrup, or any more agreeable vehicle, three mornings fasting, and then omitted three mornings, and so on alternately. In most other instances, however, the medicine has been exhibited every day, either in the morning in a full dose, or, what I prefer, a tea-spoonful three times a day about an hour before eating. The dose is graduated by the age ; grown persons take the spoon heaped full and young children proportionally. The sage should not be gathered longer than one season ; the best Jamaica ginger is preferred, and the seeds have generally been the small black, but sometimes the white have been used.

3d December.—No worms have come from him, and he has had two slight fits since the previous visit.

22d December.—Had a slight attack on the evening of the 4th and another on the 8th when the moon was full ; and on the 17th he had another from being irritated, which is the only one he has had for several months from the full to the change of the moon.

9th January 1822.—Has had five slight attacks, four before the full of the moon and one since. He was always lying when they occurred. They were unattended with convulsions, and were discovered by a frequent motion of the eyelids, otherwise he appeared as if asleep. They did not last more than five minutes, and always happened after being fatigued. His appetite has become more regular ; he is more fleshy, and his mind has evidently improved, so much so that he is able to attend to some light work, which sometimes occupies his attention nearly the whole day. Upon this subject he is regulated by his feelings and strength which vary considerably, the object being to amuse and give him gentle employment. The day after Christmas he passed "a strange looking flat worm" about nine inches long; for this is the description given of it. Supposing it might be part of a tapeworm, he was directed, in addition to the antispasmodic powder, which he has continued to use, to take early on those mornings he does not take the other medicine, a large tea-spoonful of the muriate of soda dissolved in water. It is calculated, when given in small quantities, to give tone to the stomach and bowels, to remove that state that is favourable to the production of the various kinds of worms that infest the alimentary canal, and in larger ones, to expel some of them when present. I employed it with great success in the Pennsylvania Hospital, in a case that was represented as having resisted elsewhere many of the most popular remedies for this complaint. As much was given early every morning as the stomach could readily bear without considerable nausea. In the course of the day, a great number of single joints was discharged, and occasionally pieces several inches long were removed. This plan was continued, with an occasional respite of a day, a little longer than any parts came away. I have known it to be equally beneficial in private practice.

11th February.—He persisted sufficiently in taking the muriate of soda, but no worm or pieces of one were discharged; has had four attacks since the last visit, one before the new moon and three since: they were all mild and lasted about eight or ten minutes, and he moved his body a little. His mother ascribes the recurrence of two of them to fatigue, which always produces an injurious effect on him, and to which many of the attacks are attributed. His mind seems to be completely restored, but his temper continues irritable. His strength is likewise so much improved that he works some days as well as he ever did, occupying himself in the business of a harness-maker in the shop of his father.

11th March.—Has had but three convulsions, and they were similar in violence and duration to those last reported.

22d April.—Has had three slight attacks, but they were milder than any heretofore. His mother thinks that an exposure to damp air renders him more irritable; but this state of mind is gradually subsiding. His strength improves, and he works without interruption.

7th August.—Had four convulsions during the first week in May, three of which were unusually severe; had then an interval of six weeks, afterwards a slight one in consequence of being frightened, and has since had another mild one. His temper continues to improve, and he is able to attend diligently to his work; but it is enjoined on him not to fatigue himself.

30th November.—Had no attack for rather more than three months before the 14th of October, since which time he has had three mild ones. His mother finding him so long well, discontinued, on the 6th of that month, the medicine, which he has been taking ever since my first attendance; but she again gave it on the return of the disease. When a paroxysm now comes on, he loses his strength, lies down, and stretches himself; but has not the least convulsion. In about ten minntes he rises and feels well, showing a very manifest alteration in the violence of the complaint.

30th June 1823 —Had two slight attacks about a week after the last report; took his medicine three weeks afterwards, and then laid it aside entirely. His temper has entirely lost its irritability, and his mind every trace of imbecility, and he works with diligence at his trade. This happy state lasted four years, when he went to the eastward to reside.

In this very afflicting and unmanageable disease, it is necessary to continue whatever medicine may be selected a long time; for a short period does not afford an opportunity of ascertaining its virtues. My plan has been to give the antispasmodic powder six months, and much longer if it appears to benefit. In this case it was used more than a year. I am sensible that it restores but few, but it has been more efficacious than any thing else I ever tried. Many cases that might have been relieved by an early use of this remedy, may probably continue afterwards from some organic affection, that places them beyond the power of medicine,—diseased function running into organic disorder. There are but few complaints that are cured after many years standing. In some instances, the disease has been much mitigated, although not removed, and much comfort has been obtained. I have succeeded with this prescription in some violent cases of Chorea St Viti; and in some painful affections of the head that appeared to depend on the state of the stomach, it has proved very beneficial. It acts as a gentle tonic, and sometimes as a very mild aperient, and is calculated to do good in a variety of affections that arise from the stomach and alimentary canal being deranged in their functions from want of tone.

There is one variety of epilepsy that I do not recollect having seen described by any author, which it will be proper to notice; for I consider it at least a grade of that disease. It consists in a momentary partial suspension of the faculties of the mind and the external senses, and is called in general, by the members of the family, fits of absence, from the inability, at that time, to attend properly to any thing; and is entirely different from the inattention arising from being absorbed in deep thought, or the habit of abstraction to which some persons are liable. I have seen it only in children, and, no doubt, from the transient nature of its attacks, it existed some time in them before it was noticed and considered as a disease; the want of attention being ascribed at first to the heedlessness incident to that period of life, and, so short is the absent fit after it is detected, that a great length of time passes before it is observed by the different members of the family. When addressed they do not return an answer, even when they are not occupied in any thing, and when they are apparently attending to what is said. This partial suspension is momentary, remaining in general but a few seconds, and very rarely a minute. In a very few instances, it is farther prolonged, and then becomes very easily detected. During the time, the countenance loses its intelligence, and the eye its brilliancy, and the face, more especially about the mouth, becomes pallid, without the least convulsive motion in any of its muscles. A peculiarity of aspect immediately manifests it to any person accustomed to notice it. In the milder attacks, if the child is engaged in conversation, there is a momentary cessation, and it sometimes continues afterwards on the same subject, without being conscious of there having been any interruption in what it was saying; but most generally, after the absent fit it commences a new train of thought. If reciting a lesson it will recommence where it stopped, without being sensible of any breach of continuity; at other times, it has a confused idea of something unusual having occurred. Commonly the child is silent during the paroxysm, if this term may be used to express this affection; but occasionally there is a muttering, or repetition of some sound or word. If spoken to in a loud voice, it shows that it hears, and attempts some incoherent answer. If walking, the sight is not so much impaired, but that it will move around any obstacle in its way, or even step across a gutter. If engaged in writing, the fingers often continue their grasp of the pen, and a zig-zag line is formed; or, if sewing, the work is continued but done very badly, the stitches being very irregular. A child will even, during one of these absent fits, if eating, occasionally lift the cup to its mouth and swallow its contents, but more commonly they are spilled. When affected it sometimes hurries to its parents or others for assistance. These attacks are marked with different degrees of intensity, and the external and internal senses manifest it accordingly. The same case varies during its progress, in these respects, and different cases exhibit some diversity. In some instances, the person has remained months and even years before convulsions have ensued, to mark a more decided and acknowledged state of epilepsy. These cases are generally followed by convulsions, and mostly baffle every effort to give relief. Sometimes the absent fits have entirely ceased after the convulsions have continued a good while, and in others they have occurred during the whole course of the disease. Although I have seen this peculiar state only in children, I have no doubt, as epilepsy commences at every period of life, that this affection may take place at any time of our existence. It is proper to state, that this description has been drawn from a few cases only: a greater number would have enabled me to render it more complete.

ARTICLE IV.—*Observations on Vaccination; in a Letter addressed to the Committee of the Philadelphia Medical Society appointed to collect facts in relation to the recent occurrence of Small Pox in this city. By* JOHN REDMAN COXE, M.D., *Professor of Materia Medica and Pharmacy in the University of Pennsylvania, &c. &c.*[*]

There is, perhaps, no individual in the United States—probably in the world, who has a more undoubted right to question the assertions made against vaccination, than one of the members of your committee[†]. He was himself, at the age of three weeks, one of the earliest of those subjected to its influence, now upwards of twenty-six years ago, and was, immediately afterwards, subjected to every possible means of variolous infection. Inoculated during a succession of years, probably at least fifty times by myself and subsequently repeated by his own hands, he was likewise exposed for a considerable time, and held in the arms of an individual in the most malignant state of small pox, not more than ten weeks after the termination of the vaccine. This, together with numerous other trials on the subject, made with every possible care and precaution, many of which are detailed in my treatise on vaccination, published in 1802, fully satisfied my mind, that the vaccine was entitled to every praise as a preservative against small pox, which had been ascribed to it by the illustrious JENNER. All of them triumphantly and fully substantiated the anti-variolous character of the vaccine; and I have not the slightest reason for believing, that any case that has thus come under my care, has been affected subsequently with what is termed the varioloid. The only solitary instance of any approximation thereto, was in a boy, about five years ago, whom I vaccinated in 1802, and who then had an eruption, said to be small pox, but which I had not an opportunity of seeing, until the decline of the disease. The attending circumstances, however, inclined me to believe it was the chicken pox; although, admitting it really to have been small pox, on referring to my minutes of the case, I find it stated to have been perfect, but particularly mentioned as having been much rubbed. Nevertheless, it probably may have been of the nature of small pox; for it appears from the same minutes, that in ten weeks from vaccination he had urticaria, and in two weeks from that period, chicken pox, which was communicated to his nurse, who had had the small pox in early life. I am not now, nor was I at that time, at all disposed to question the efficacy of vaccination from this case. Its having been much rubbed is probably an adequate reason for presuming it had not sufficiently guarded the system, and I did not test this case by variolous inoculation.

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[*] [We published some extracts from this interesting letter in our last number, as a part of the report of the Committee on small pox; but the whole is so highly important, and comes from a source so justly regarded on the subject of vaccination, that we conceived that we could not do a more acceptable service to the profession than by laying it before them entire. The author readily acceded to our wishes, and had the goodness to furnish us with a revised copy of the original communication with additions for our use.—*Ed.*]

[†] Dr EDWARD JENNER COXE.

In referring to my minutes, which I kept during the whole progress of my experimental researches into the character of the vaccine, with the most scrupulous exactitude, and *daily* reported *every* individual case that came under my care, I find I have uniformly noted the term *perfect*, at the end of each case, with which I was satisfied, and that of *doubtful*, where any circumstances gave the disease a suspicious character; so that I can now advert to every case I had for the first three or four years, with the certainty of finding every particular attending on each. After that period, I was less attentive in this respect; but I was minutely attentive to seeing my patients, *at least every other day*, during the *whole progress* of the disease. Few, I believe, can say the same, and fewer still can point to *any certain record of the real character* of the disease, which may be noted in their daybooks as the vaccine! This can be adduced at this distant date, only in proof of the insertion of the matter: of the future progress of the infection, little can be known. Memory is too treacherous to be regarded as evidence in a case of this description, by which the vaccine must be forced to forego its asserted prophylactic powers; and I would infinitely rather presume some absolute imperfection or failure of the process, than give up my firm conviction in its preservative tendency.

To evince that even at that early period of vaccination, I was strongly impressed with an apprehension of the dangers likely to ensue upon the inattentive manner in which the process of vaccination was too frequently pursued, owing to its mild and gentle character, I beg to quote the following extract from my treatise adverted to. It refers to the necessity of constant and daily attention to the disease throughout its course, and the want of which is, I firmly believe, one principal cause of the present uncertainty we labour under respecting it. "Enough is said, however, I trust, to point out the necessity of strict attention, in order to prevent our being imposed on by the *spurious* for the *genuine* disease; an error, which, by the false confidence impressed, has unfortunately been productive of consequences fatal to the patient, and of injury to the extension of this most valuable blessing. That this is not a false dread, must be very evident to the inhabitants of such places, into which the spurious disease has been unfortunately communicated. *Even here*, where the knowledge of the disease has been more extended, and is more easily acquired, it has been insufficient to prevent the occasional appearance of this insidious enemy, which has not failed to check the favourable impression of the disease which many had previously entertained."—Preface, p. 8.

That many mistakes occurred in the early period of vaccination, few will, I believe, doubt; and that they have been, and will continue to be, perhaps for years to come, a fertile source of the supposed or real cases of small pox after vaccination, have long been firmly established truths in my mind, and must prove to physicians the absolute necessity of paying much stricter attention to the disease, than has, I fear, been usually done. The mildness of the vaccine has often been the occasion of physicians neglecting to see their patients regularly; by which means, circumstances have been overlooked, that might have produced some degree of doubt as to the perfection of the disease they had excited, during that period, in which its regular progress ought to have been sedulously watched and carefully noted down; so that memory might not be charged with a recollection of events long passed by, and which may be deemed to be impossible, even in a person possessed of this faculty in the most tenacious degree. A physician, for instance, sees a small and apparently perfect vesicle following the insertion of the virus, on the fourth or fifth day. Satisfied in his own mind, he, perhaps, omits his visits for several successive days, during which interval, an accidental injury may have completely changed the character of the disease. He finds a creeping sore, with an irregular scab progressing, in place of the perfect and beautiful vesicle he had anticipated. From its former appearance, however, he persuades himself that the disease has produced its perfect effect upon the system, and pronounces his patient free from any danger from variolous infection. But in this early stage at which an accident may occur, how can he be certain that the disease, then *merely local*, may not have completely lost its peculiar property, and become a sore entirely different from the perfect pock that runs its regular course! Such cases I have seen, and such I have always regarded as at best *doubtful*, and in whom the after occurrence of small pox would not by any means surprise me.

That each case of the disease, however perfect and regular in its progress, may not *invariably* prove a preservative to the system, will be readily granted, if the disease does not affect the system constitutionally; or, in other words, if it should be merely a *local* affection. There can be no reasonable doubt, that such may occasionally be the case; and it is, perhaps, as yet a defect in vaccination, that we have no absolute and unqualified test, in *every instance*, to ascertain the constitutionality of the disease. This, however, derogates in no degree from the value of vaccination, but merely proves our imperfection as to all its particulars. The best test, so far as my experience warrants me to state it, is the one recommended by Mr Bryce, that of re-vaccinating about the sixth day. In this case, if the re-vaccination succeeds, it progresses with equal rapidity and with a character as strongly marked, although inferior in size, as that of the original vesicle, provided the system has been pervaded by its influence.

However this may be, there is sometimes the same uncertainty in the small pox itself. One superiority, nevertheless, the vaccine possesses; viz. that no other disease can be mistaken for it. It possesses a character not to be mistaken; whereas the chicken pox and measles, &c. have at times been mistaken for the small pox, or the reverse*. Neither have we a modified vaccine, as far as I know; accident may destroy it, but no other disease seems to assimilate with it, and derange its appearance, to the danger of its being mistaken. It may vary as to colour, being sometimes quite blue as indigo, and the appearance of the lymph is sometimes clouded; but the general aspect of the vesicle is so peculiar, as scarcely, except from accident or absolute ignorance, to be mistaken. A person acquainted with the character of the vaccine can only be mistaken in it from accidental injury; for it differs, toto cœlo, from every other known eruption.

At the hazard of encroaching on your time, I shall cursorily mention a case of genuine vaccine followed by small pox, which, if not explicable on the above supposition of its being merely local, may yet be explained on the ground of its being one of those few cases, which might have been liable to a second attack of small pox. Nor is it more extraordinary, that an *occasional* instance should occur of small pox *after* vaccination, than that now and then a case of vaccine should take place after perfect small pox, of which I have known and recorded about a dozen instances.

—— G. was vaccinated by Dr —— March 31st, 1803. The vesicle went regularly through its course *without inducing any indisposition*. The areola was perfect on the 9th day, and the scab fell off on the 22d. On Thursday, three weeks from vaccination, viz. April 21st, the child was exposed to the small pox, in a patient having it very finely on the turn. On Friday, the 29th, the child became indisposed; this continued on Saturday and Sunday, when a scarlet efflorescence appeared over the body, and, on Monday night, the child had two convulsion fits. On Tuesday, several pock appeared over the body and face, continuing to come out for three days: about twenty filled and maturated. The face became tumid, and the eruptive fever disappeared. The child had had the chicken pox during the winter. I did not myself see this case, but have no doubt of the accuracy of the information.

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* Many of the old writers on small pox have described several varieties, as they deem them, under the names of horn pock, wind pock, and water pock; and MORTON speaks of the chicken pock as a species of the disease.

During our Lectures of 1825–6, I was called to attend Mr Sawyer, a student, son of Dr S. of North Carolina. Had he not assured me he had been vaccinated in early life by his father, I should have at once affirmed he was about to have the small pox. This satisfied my mind, but he nevertheless had the disease in high perfection; not a modified case, but the real old-fashioned disease. Of the preceding vaccine, I know nothing.

Twenty-six years have just elapsed, since I first succeeded in introducing the vaccine disease into Philadelphia. A new generation has within that period sprung up, who owe their safety from the small pox to the former disease. If we suppose the population of Philadelphia to amount only to 120,000, we shall probably not err, in stating the vaccinated portion at one-half. We have had, in the period named, the small pox several times amongst us; but, although the inoculation for that disease has been long suspended, except as a test after the vaccine, we have never seen it spread amongst that immense number, who have been vaccinated! Is it possible to imagine that one-half of a city, like Philadelphia, could have so entirely escaped the ravages of an epidemic, if they had not securely opposed the egis of JENNER to that loathsome and dangerous disease? Where are now the numerous vestiges of its frightful ravages? It is now as *rare* to see a person disfigured from the small pox, as it was *common* formerly. If some have taken what has been called the varioloid, what a small proportion do they bear to those who have escaped. Of those who thus have taken it, I must require accurate proof, from written notes taken at the time, that the vaccine had *really* been taken, and that it had regularly passed through all its stages. But where are these proofs? What documents can our physicians generally produce of such high and obvious importance? Will they appeal to memory. This is too treacherous to command belief in matters of distant date, and especially as vaccination has been too frequently pursued. For my own part, I have little doubt, that by far the greater number of those, who have had the so called *varioloid*, *modified* small pox—or by whatever other name it may be called,—actually laboured under the *genuine* disease, from vaccination imperfectly performed, or gone through.

It will, I think, be found that the description of varioloid is very similar to that of many of the cases of small pox, as laid down by the old writers on the subject; and more particularly of some of those anomalous ones, which were apparent in almost every epidemic described. It is well to remember, that few physicians, now living, have ever witnessed such an epidemic; and our knowledge must consequently be derived from a close comparison of our so called cases of varioloid, with the accounts handed down to us by those who have witnessed small pox, in every shape and feature. Let such a comparison be strictly instituted between the accounts left us by SYDENHAM, MORTON, and writers even preceding them, and those of *varioloid* and *modified* small pox of the present period, and a strong analogy will be found to exist between them. I shall be glad, myself, to know what peculiar symptom now occurs, that may be regarded as absolutely diagnostic of these so named disorders.

It is time for physicians carefully to attend to vaccination throughout *its whole* career, and minute down each case; if they desire fully to comprehend the character and progress of any future disease, which they may designate by terms that appear to me unfounded. This is a duty they owe to themselves and to the public; and would be a great relief to that state of uncertainty, under which the mind now labours. For myself, I can only say, that no circumstance has yet tended to diminish my firm belief in the efficacy of vaccination, when faithfully and accurately performed, and duly attended throughout its full career.

I think it might be well to establish by law, that every physician, who vaccinated a patient, should keep an accurate record of the case, and minute down each day's appearance of the disease. It may be said that this would be too troublesome and laborious for the physician in full practice. It is nevertheless, I think, an imperative duty; and would at all times be a sure reference, in case of future alarm as to varioloid, &c. If the labour is deemed too great, let the business be committed to a few, who will undertake thus to record their practice in this disease; and let it be the duty also of the physician, at the close of the disease, to deliver to the patient or his friends, a faithful transcript of his daily remarks; so that, in case of the death of the former, no uncertainty may remain as to his sentiments at the time, whereby to establish the real character of any future eruptive complaint that may overtake the patient.

I will only further remark, that however proper it may be to enact a law prohibiting inoculation *in the first instance*; it is altogether unnecessary, if not absolutely incorrect, to prevent physicians from testing their patients by it, subsequently. Either the vaccine is, or is not, the preservative it is affirmed to be. If it is, what hazard can attend this subsequent trial? If it is not, we ought as speedily as possible to renew the system of inoculation, as inferior in danger to the natural disease.

ARTICLE V.—*Observations on the Employment of the Catheter or Sound for arresting Hemorrhage from the Urethra; with Remarks on the probable Modus Operandi of the Instrument. By* WILLIAM DARRACH, M. D.

[With a Plate.]

The case of severe hæmaturia, noticed in the Medico-Chirurgical Review, (vol. xi. No. 29, p. 145—6), in which a sound was employed by Dr CHASTAINGT to arrest the hemorrhage, reminded me of a case of profuse hemorrhage from the urethra after lithotomy, in which the same means were successfully employed.

An individual, who had been afflicted for some years with symptoms of the stone, applied to Dr PHYSICK in the fall of 1817, and underwent the operation of lithotomy. Considerable difficulty was experienced in passing the sound and finding the stone. The hemorrhage from the transverse perineal artery was unusually great; its calibre being equal to that of the pudic of ordinary size, and requiring to be secured by a ligature. The stone, which was large, was found partially incysted in the anterior portion of the bladder, a little above its neck. The patient was put to bed in the morning and passed the rest of the day comfortably. The next morning, however, a profuse hemorrhage occurred, *the discharge of blood proceeding wholly from the urethra, and not in the least from the wound.* Dr PHYSICK, having arrived, requested a catheter of the largest size, and, instantly passing it into the bladder by the urethra, had the satisfaction to find the alarming hemorrhage permanently arrested.

Though Dr CHASTAINGT introduced the sound with other intentions than to arrest hemorrhage, and though happy accident favoured him with an *unexpected* cure; yet the profession will not be the less pleased to receive his case, in proof of the excellence of an *apparently new* practice. The instruments employed by Dr CHASTAINGT and Dr PHYSICK were different; the one a sound, the other an elastic gum catheter. The complaints nosologically considered were also different, the one hæmaturia, the other consecutive hemorrhage from lithotomy; yet in essential points, there was no difference between the two cases. In both, a sudden, profuse, and alarming hemorrhage occurred from the urethra, and was arrested by the introduction of a large urethral instrument.

Remarks.—Under this head, we shall inquire what was the seat of the hemorrhage in these cases, and what the modus operandi of the catheter and sound.

The review, above quoted, states, that Dr C., after having submitted the case of his patient to the physicians and surgeons of Paris, determined to examine the state of the bladder himself. Accordingly he introduced a sound, which met with some resistance at the neck of the bladder. This being overcome, the cavity of the organ was explored, but nothing could be detected. Dr C. now imagined that the resistance to the sound might have been caused by *varicose veins*, which probably were the seat of the hemorrhage. He, therefore, allowed the sound to remain in the bladder to exert a *salutary pressure* on the supposed dilated vessels. After this operation, the patient made water unmixed with blood.

Dr PHYSICK's case also presents matter for conjecture. It may be asked, what furnished the blood? In stone cases, secondary hemorrhage takes place generally from the wound, and proceeds from the superficial perineal, transverse perineal, inferior and internal hemorroidal, or pudic artery, or several of these vessels at the same time. But is there a case on record, besides the above, in which secondary hemorrhage happened *exclusively* from the urethra? What is the source of the hemorrhage in this case? If it be the divided surfaces of the prostate, rendered morbidly vascular by an irritating paroxysm of stone, why was there no hemorrhage by the wound? If it be fungus, or vascular flocculency of the mucous coat of the bladder, or enlarged and ruptured vessels of that portion of the surface of the viscus, where the stone adhered, why did the blood flow exclusively from the urethra? And finally, how are we to explain the fact, that the passage of a catheter should have so *quickly* and *effectually* arrested the hemorrhage?

None of the places above enumerated could have been the seat of the hemorrhage in this case. We are disposed to think, that its seat must have been in the ***congested veins*** of the urethra ; and surgical and anatomical knowledge will be found to support the conjecture.

Pelletan (tom. ii. p. 283,) remarks that a varicose state of the veins at the neck of the bladder is a very common occurrence in ***stone*** patients, particularly if aged.

Boyer (tom. ix. p. 95,) enumerates, among the causes of hemorrhage from the urethra, varicose dilatations of the vessels at the neck of the bladder which become ruptured ; and adds, that, as this hemorrhage is sometimes preceded by suppression of hemorrhoids, some have imagined that this suppression produced a particular determination of blood to the vessels at the neck of the bladder, causing, what they have been pleased to call, hemorrhoids of the bladder.

Chopart, in autopsic examinations, has seen the vessels of the bladder varicose, and very tortuous under the mucous coat.

Morgagni has observed the same. **Bonet** (Sepulc. vol. iii.) describes a case of discharge of blood from the urethra, in which a ***redness remained*** after death about the neck of the bladder.

Desault (vol. iii. p. 39,) is yet more to the point. We prefer to give his observation in his own words, as it shows that the method so successfully employed by Drs **Physick** and **Chastaingt**, and original with them, was long before practised by him. He says, " Persons who reside any length of time in hot climates are particularly liable to be affected with bloody urine, having its seat in the ***varicose vessels of the urethra*** and neck of the bladder. I have treated and cured this complaint in several soldiers, returned from the East Indies, by means of the ***elastic catheter****.

The commonly received anatomical views of the urethra and bladder do not explain the phrase, ***varicose vessels of the urethra***, used by **Desault**. We can understand the expression, varicose vessels of the neck of the bladder ; but where are the varicose vessels of the ***urethra*** situated ? The urethra consists of a delicate membrane, together with a structure called the ***spongy body***, to distinguish it from

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* " Les personnes qui habitent les pays chauds sont particulierement sujettes au pissement de sang, provenant *des vaisseaux variqueux de l'urétre* et du col de la vessie. Nous avons traité et guérié de cette maladie au moyen de *sonde elastique*, plusieurs soldats revenant des grandes Indes."

Fig. I.

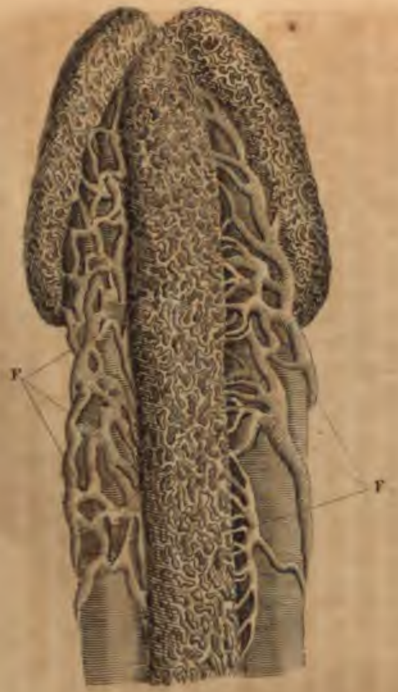
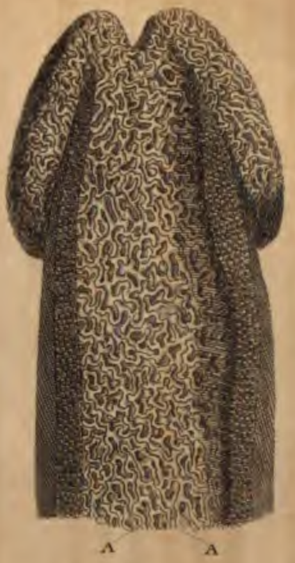


Fig. II.



the other portions of the penis, denominated *cavernous bodies*. No one thinks of making the delicate lining membrane of the urethra the seat of varicose vessels; nor will the commonly entertained idea of the intimate structure of the spongy body admit of the supposition that this is the seat of them. Between these two structures, which are united merely by cellular tissue, no distinct set of vessels has been discovered. Where then are we to find the varicose vessels of the urethra, spoken of by DESAULT, and are we to consider them as veins or arteries?

HUNTER and MASCAGNI's views of the corpus spongiosum urethræ afford a satisfactory answer to the above question. The *varicose vessels of the urethra* can be nothing else than *congested portions of the corpus spongiosum itself.*

The corpus spongiosum is a venous plexus, and is as much a *constituent part* of the venous system as the spinal plexus, or any other venous plexus of the body. It has been arbitrarily separated from this system, because it forms a distinct body, which, with three others, namely, the glans and two corpora cavernosa, compose the entire penis, and because this organ became an object of very attentive study and examination by anatomists, long before the entire venous system was made out or fully investigated. Now, the corpus spongiosum, or in other words, *urethral venous plexus*, communicates directly with a venous plexus situated on the corpora cavernosa; and this latter, by means of the vena azygos, with a venous plexus situated at the neck of the bladder externally. But MASCAGNI's description of the structure in question, with the assistance of the plate, which is taken from his "Prodromo," will present the subject more clearly.

Figure I.

FF "The trunks of veins branching from the deep seated dorsal vena azygos of the penis: which, traversing tortuously and with numerous anastomoses the corpora cavernosa, proceed laterally to the urethra, to penetrate *the compact venous plexus*, which composes the *corpus spongiosum* and the glans penis. These trunks unite with this venous plexus, and form together the intimate structure of the spongy body."

Figure II.

AA "The internal surface of the canal of the urethra, deprived of its lining membrane. It appears rough, and is covered with ridges and fissures of various dimensions, which are an assemblage of *veins* variously interlacing each other, and having interposed their little accompanying arteries."

If, with MASCAGNI, we regard the structure outside the delicate urethral membrane, to be a *tissue of contorted venous tubes*, each of which is liable to temporary morbid dilatation, or permanent varix, as venous tubes elsewhere in the living body ; it will not be difficult to understand how congestion may exist here, and consequent rupture take place, when circumstances, attendant upon the existence of stone in the bladder and its extraction, cause an undue determination of blood to the genital system.

Nor will it be difficult to explain the modus operandi of a large sized catheter or sound, in arresting a hemorrhage from the spongy body, on the supposition that it is a *venous plexus* in a *congested* state. The lining membrane of the urethra is very dilatable: the strongest evidence on this point has of late been furnished by the Lock Hospital practice in strictures, and by the operations of CIVIALE. The instrument, used in case of hemorrhage, stretches this membrane, so as to cause it to press against the spongy body, in other words, the venous plexus. This is pressed upon also by its external fibrous covering; and the consequence is, that the plexus becomes emptied of its blood, which, being forced into the deep seated communicating branches of the dorsal plexus, passes through these into the azygos, and finally into the vesical plexus. The source of the hemorrhage is thus stopped, and time is afforded for the emptied and ruptured vessels to heal, and the debilitated and enlarged exhalants to regain their tone.

ARTICLE VI.—*Observations on the Medical and Chemical Qualities of the Cornus Circinata, or Round-leaved Dogwood. By* JOSEPH H. ROBINSON, M.D., *of St Croix.*

The genus Cornus is distinguished by the following characters: Flowers, sometimes aggregated in a four-leaved involucrum. Calix, 4-toothed. Petals, 4, small, broader at the base. Drupe, inferior, not crowned by the calix. Nut, 2-celled, 2-seeded.—*Nuttall.*

The following are the species: 1. canadensis—2. florida—3. circinata—4. sericea—5. asperifolia—6. stricta—7. sanguinea—8. alba—9. paneculata—10. alternifolia. Of this genus there are also two other species in Europe and Asia, and two which are common to the old continent and America.

The circinata is designated in many parts of America, by the familiar appellations of *mountain willow*, *white rind*, *and round leaved dogwood*. Perhaps it may have derived its name from the shape of its leaves, (circinatus, rounded), as they are more oval than those belonging to the other species.

The circinata appears, from the above description, to be connected with a numerous kindred of plants, belonging to the class tetrandia, and order monogynia. It is not remarkable for its dimensions or beauty. It inhabits the sides of rivers; but is partial also to elevated and mountainous situations. Its localities are pretty numerous: it dreads, however, the coldest latitudes, as much as it dislikes the warmest. It may be found in many parts of that extensive tract of country, extending from Canada to Virginia. It is a shrub of eight or ten feet in height, and its foliage is not particularly thick. Its leaves are oval and broad, white and downy beneath, and waving at the edges. Its branches are rough and warty. About the middle of the year, it produces a small white flower, and berries of a blue colour. It is distinguished by PURSH in the following scientific language: "Ramis verrucosis, foliis lato-ovalibus, acuminatis, subtus albo tomentosis, cynus patentissimus."

The branches and stalks, when prepared for pulverization, have rather a quilled appearance, whitish internally, and possessing a dark epidermis. After the bark has been pulverized, it assumes a light yellow colour, has an aromatic flavour, and a pleasant, bitter, and astringent taste; although when first put into the mouth, it has the peculiar taste of colombo.

The circinata has not yet attracted much attention from the Faculty. It is mentioned in the pharmacopœia of the United States, as an inferior article; and is very cursorily noticed in some of our most valuable works on Materia Medica. From various circumstances, with which I have become acquainted, in connexion with the history of the plant, I am induced to believe that professors MONSON and IVES, of New Haven, are entitled to the credit of having first introduced it into general practice. Recently, however, it has been used by the learned editor of "Paris's Pharmacologia," and is mentioned by him in the most decided terms of approbation. Like some other medicinal agents, its properties were discovered by mere accident. The following interesting account of its discovery and early use is related by the worthy and much respected professor of Medicine in Yale College. "The Cornus circinata was formerly kept as a nostrum by a person from Canada, who used it with success in New Haven in 1795. A patient affected with a diseased liver was attacked with a diarrhœa, which it was not in the power of the physicians to check. At the request of the possessor of this nostrum, the physicians consented to give it a trial, and the diarrhœa was very much relieved by its use." "These facts," continues Dr Ives, "I had from Dr Æneas Monson. From the use of the remedy in this case, Dr Monson entertained a very high opinion of the article, and requested me to inquire for it. In 1803 I went several times upon the mountains, in quest of the plant. By an old man upon the mountains in Hamden (a place in the neighbourhood of New Haven) I was directed to the place, from whence it had been gathered by the empiric. I found the plant described in Willdenow, under the name of Cornus circinata. Its locality is on the Whenstone mountains on the north western aspect, among the broken rocks, and is found in this situation upon the east and west mountains in this place, (N. H.), locally associated with the Sambucus pubescens, and in abundance."

My own experience has not afforded a very extensive knowledge of the therapeutic character of the circinata. In my own case, however, such was the beneficial operation of the remedy, and so high is the opinion entertained by several eminent practitioners of its powers, that I shall never hesitate to employ it in all diseases of the bowels, attended with a weakened state of the mucous membrane.

Shortly after my arrival in America from the West Indies, I was seized with a violent and distressing diarrhœa, which, from the appearances of the evacuations, the state of my liver, and various other circumstances, I was induced to believe, proceeded from some derangement in the biliary apparatus. When the disease first made its appearance, I neglected all medical treatment; but after some time had elapsed, I was obliged to resort to an extensive use of cathartics, alteratives, and tonics. But they produced temporary alleviation only. In fact, the powers of the far famed colombo were never found more inefficient. My mind now became a prey to despondency, and my spirits began to lose their natural elasticity. From the sudden attacks of the disease, I could never enjoy any necessary recreation, or find any satisfaction in the society of friends. In this dreadful situation, I consulted professor Ives, and was advised by him to try the circinata. I did so, and I am happy to state that the most satisfactory results attended its employment.

In other cases of a similar nature, the Doctor has used it with unvaried success. "In diarrhœa," he observes, "such as affects the sailors coming from the West and East Indies, it has exhibited itself superior to any other tonic and astringent, and is much less disposed to produce congestion than the Peruvian bark." The Doctor also gives his preference to this article in the diarrhœa attendant on pulmonary consumption, as well as in that which occurs in the latter stages of typhus, particularly in those cases which are liable to terminate in hemorrhage from the bowels.

This distinguished practitioner has derived considerable advantage from it, in the form of decoction, in chronic affections of the liver,. accompanied with dyspepsia, when "thirst is present, and other liquids oppress."

In cholera infantum, the same gentleman has used it with decided benefit. He gives his testimony to the virtues of this remedy by observing, that "for a number of years he has used more of it than of the Peruvian bark."

In dysentery, after the proper preliminary steps have been pursued to lessen the inflammatory state of the bowels, and when the disease has assumed a chronic character, it will be found highly efficacious. In confirmation of this, we possess the pleasing assurances of Dr Ives of New-York. He remarks, with regard to its general exhibition in his practice, that "his experience in the use of this article has been sufficient to persuade him that it is a pleasant and powerful tonic, and that there are cases in which it is preferable to any other vegetable, belonging to the class of tonics."

Being under the conviction that it possesses highly tonic properties, and feeling satisfied that it can be retained more easily upon the stomach than the cinchona, I conceive that it may be used with much advantage, as a substitute, in the various forms of fever to which that heroic medicine may be suited. This remark is particularly applicable to the cases of children, for the reasons which I have assigned above.

In erysipelas, when marked by a feeble and typhoid state of the system, I have no doubt this bark will prove serviceable, especially in this country, as the disease frequently proceeds from the depraved habits of debauchees and drunkards. From these causes, the stomach is generally found extremely irritable; and as the circinata possesses a more aromatic flavour than the cinchona, I have no doubt it would be retained better, and produce less nausea.

But the circinata has not evinced remedial effects, merely when exhibited internally. Dr Ives of New Haven assures me, that he has used it "as an external application for gangrene, in preference to the Peruvian bark, and for ulcerated and spongy gums." In such cases, the Doctor adds, "it has in my hands succeeded better than any other article."

Through the friendly exertions of Mr Carpenter, a most intelligent druggist of this city, and a gentleman well known to the medical faculty from his interesting analyses of various substances, I have been enabled to obtain a knowledge of the constituent parts of the circinata. From his notes of the analysis, the following observations have been culled:

With *water*, the bark produces a deep red colour.

Proof spirit forms with it a beautiful tincture, and dissolves the largest portion of the active principles. It may consequently be considered as its best menstruum. The tincture furnishes, on evaporation, a dark red *extract*, which possesses, in a concentrated degree, all the bitterness and astringency of the bark.

To *alcohol* it imparts the same appearance as to water. The alcoholic solution, when concentrated by evaporation, is rendered milky by the addition of water, and a precipitate is formed. The *extract* digested in ether is partially dissolved, and the solution, on evaporation, furnishes a compound of resin, oil, and a small portion of a peculiar saline matter. This compound appears, from its sensible characters, to constitute the most active portion of the article.

In the action of reagents, the circinata affords a striking similarity to the cinchona. The *watery infusion* reddens tincture of litmus. Muriatic acid forms a yellow flaky precipitate with it. Acetate of lead in solution was quickly and copiously precipitated. A solution of tartar emetic was rendered turbid, and slowly precipitated. Sulphate of iron rendered it turbid, and immediately afterwards changed it to a bluish-black colour, and produced a very light precipitate. With lime-water a very copious precipitate was formed, leaving the supernatant fluid almost colourless. Sulphuric, nitric, tartaric, prussic, and acetic acids were severally tried, but evinced no action on the infusion. The solution of ammonia caused no precipitate, but the infusion assumed a deeper red colour.

It will be perceived that the action of the above reagents is the same on the circinata and cinchona; but a difference is apparent, when the solution of galls or isinglass is used; the cinchona being copiously precipitated by either, whilst no change is produced on the infusion of the circinata.

From the above statement we may conclude, that the constituent proximate principles of the circinata are, tannin, gallic acid, resin, gum, mucilage, oil, and a peculiar saline matter, which differs from the salt discovered in the cornus florida, in possessing less bitterness, and more astringency.

From the examination of this substance, I am induced to believe that the extract will prove a valuable preparation. At present the bark is used in substance to the extent of one or two drachms, or in infusion and decoction, in ounce or two ounce doses. The infusion is prepared by adding half a pint of water to two drachms of the pulverized bark, or by pouring a pint of boiling water on one ounce of the bark coarsely powdered. The decoction is made by boiling one ounce of the bark in a pint of water. By professor Ives, the powdered bark has been employed in a composition, similar to the compound tincture of Peruvian bark, the circinata being used as a substitute for the cinchona. In obstinate diarrhœa, accompanied with dyspeptic symptoms and dry skin, Dr Ives of New York recommends a combination of one drachm of the bark with two or three grains of ipecacuanha, and given pro re nata. I have myself used one drachm of the powdered bark, mixed with two teaspoonfuls of the best Port wine. Externally, the circinata is used in fine powder.

ARTICLE VII.—*Kentish's Ointment recommended in the Treatment of Erysipelas. By* CHARLES D. MEIGS, M.D.

Regarding the blood vessels of an inflamed part as in a state of diminished contractility, I suppose the best manner of curing inflammations, will be found to consist in restoring their contractility, so far as to bring it on an exact par with that of the contiguous or neighbouring healthy vessels. Sometimes we can effect this desirable object by very simple mechanical contrivances : a roller, a compress, position, &c., are in many instances the only things requisite to bring about this equilibrium. M. BRETTONEAU, of Tours, in France, cures even a bad scald in the course of a very few hours by a roller, which, acting as a substitute for the natural power of the capillary vessels, prevents not only their being dilated by the forcible injection *a tergo*, but also all the consequent phenomena of that sort of inflammation, called a scald or burn. Mr BAYNTON cures the most inveterate sore legs by strips of adhesive plaster and rollers, and every body knows how rapidly an œdematous limb may be brought back to a proper size and shape by a common roller judiciously applied. All these effects are dependent on the improved power of resistance, which they occasion in the weakened vessels of such parts.

It is quite clear that we can by mechanical means make an excellent substitute for the natural contractility of the small blood-vessels in superficial inflammation. Some persons say that this external pressure is the *true antiphlogistic.*

These plans are useful in procuring the resolution of inflammation. So, also, when we apply various washes, cold poultices, pledgets of vinegar and water, &c., we have it in view to promote the contraction and emptying of the vessels ; for all those means occasion the parts to become whiter and paler, and the more so, as they are more efficacious, (i. e.) as they occasion the blood to be more completely excluded. But cold, lead water, &c. are not the only things that possess the power of increasing capillary contractility. Mr KENTISH has shown, that in burns nothing possesses so much power in this way as his famous ointment, which for its excellent qualities in the cure of burns and scalds is known over the civilized world.

I have, for some few years past, employed the same ointment to dress the erysipelatous inflammations that have fallen under my care; and I venture to affirm that I have found equal success from no other mode of treatment, though I have tried a great many.

There is a sort of erysipelas which affects children, and which has in not a few instances proved fatal. It is a most painful and unmanageable disease by the ordinary methods; yet I have seen it yield with the greatest rapidity, manifestly from the favourable action of Kentish's ointment. I had a case not long since, in which a child had violent convulsions, in consequence of the constitutional irritation, produced by an erysipelas occupying the whole of its right foot, leg and thigh: the free application of the ointment removed the disease with the greatest readiness. I have treated a great many cases in this manner and have always found the application not only useful, but soothing and delightful to the patients. Instead of making extensive incisions, as some of the moderns have recommended, I would respectfully urge my professional brethren to make trial of this ointment, which is both easy to administer, and exceedingly comfortable to the patient.

I need not enlarge here on the similarity, I might say identity, of spontaneous erysipelas, and that occasioned by the application of heat. Both inflammations are cutaneous, diffuse, smarting, and disposed to vesicate. Kentish's ointment is as useful in the one as in the other, and no disease yields more surely to any medicinal application than both of these do to the spirit of turpentine and basilicon mixture.

My method of employing it, is to render basilicon ointment soft (not fluid) with spirit of turpentine, and then to rub it on the part with the fingers. Let the anointing be repeated often enough to keep the part always very thinly covered.

This application does not interfere with any constitutional treatment, which may be demanded by circumstances. All such treatment is, however, unnecessary, in a majority of cases.

One of my patients has just recovered from a severe œdematous form of the disease in the leg, which has for years been affected with an ulcer, by the sole employment of the above mentioned ointment.

Analytical Reviews.

ARTICLE VIII.—*Pyrétologie Physiologique; ou Traité des Fièvres, considérées dans l'esprit de la Nouvelle Doctrine Médicale. Par* F. G. BOISSEAU, D.M. *&c. &c. 2-ième Edition. Paris*, 1824.
Nosographie Organique. Par F. G. BOISSEAU, D.M. *&c. &c. Tome I. Paris*, 1828.

THE modern French medical doctrines are so often referred to, are so valuable, and, at the same time, so imperfectly known among us, that we have thought it would be a useful occupation of a portion of our pages, towards at least a large portion of our readers, to present an occasional sketch of some of their elementary parts. Of these, several have been occasionally taken up in our former numbers; but we have, as yet, offered no account of the prime subject of gastritis, as connected with fevers. Without something of this kind, to those who have not access to the French works, our inquiries must be incomplete; and we eagerly embrace the opportunity offered to us by the new volume of M. BOISSEAU. Together with the Pyrétologie of the same writer, it affords an excellent opportunity of exhibiting, so far as can be done in the crowded leaves of a review, the very points which we conceive are wanting.

The "Pyrétologie Physiologique" is an analysis of the nature of fevers, upon the principles, as apprehended by the author, of the physiological school of medicine. For this object, M. BOISSEAU has resolved to assume the ordinary definitions and descriptions of fevers, as commonly received by his countrymen, and bearing a considerable resemblance to those in use among us, which, as is well known, are mainly derived from the school of CULLEN. By so doing, he conceives, we think with justice, that he is greatly facilitating, and rendering less repulsive, to the mind those modes of investigation and new facts, contributed for the improvement of science by M. **Broussais**, and various other labourers, many of whom have arisen under his auspices. To be furnished with an elaborate description and discussion of the characters and treatment of a variety of phlegmasiæ, principally of the primæ viæ, and to wind up by an attempt to demonstrate that "there is no such thing as fever," may be a correct synthetical process; but it requires such a sudden and revolting change of habits of thinking, that it is more likely to shock and discourage a practitioner, than to add to his amount of serviceable knowledge. On the other hand, prepared as at least so many of our countrymen are for the idea, that there exists, as we phrase it, "no idiopathic fever," we conceive the analytical order much more likely to prove useful. It is in vain to tell us, that "there is no such thing as a bilious fever;" we are, unfortunately, too well aware of the contrary: but if we are told, that "bilious fever" is entirely owing to inflammations of certain viscera, and that the knowledge of this fact will enable us materially to improve the treatment and our own success, we are immediately anxious to know why it is thought so, and are quite ready to peruse the necessary cases, dissections, and arguments.

Dr **Boisseau** has adopted the classification of professor **Pinel**, the arrangement exclusively in use in France, at the epocha when Dr **Broussais**'s writings first appeared.

"It was time to connect with the great body of science, the researches which have been made within a few years relative to the seat and nature of fevers. A physician who wishes to describe the present real state of our knowledge on this subject, must take, for his starting point, the work of M. Pinel. Any other order than this, in place of diffusing clearness and light over the opinions of practitioners, and presenting a faithful guide to students, placed as they are between two contending systems, could produce no other effect than to confuse the ideas of the first, while it left the latter in a painful uncertainty, suffered them to fall into an unjust and inconsiderate contempt of one or other of the two doctrines, or what is still more dangerous, induced them to imagine that medicine has no fixed principles, and that it is nothing but a fluctuating product of the imagination."

"A new epocha," says our author, "has commenced in the science of medicine: yet our task is not to reconstruct the edifice of science, but to add to it, and renew, some of its parts which, from their antiquity, have tumbled into ruins."

The description of the state of medical parties and opinions in France, given to us in the preface to the Nosographie, is so interesting, that we will venture on an additional extract. Speaking of the appearance of the doctrines of professor Broussais, Dr B. remarks:

> "Until then the French physicians had generally lived in a species of harmony, somewhat rare among men of our profession; but, in 1816, they separated into several parties. Some, proud of having resisted the enthusiasm inspired by Pinel, remained faithful in their admiration of the past; others, superior to all personal feeling, adopted, with certain restrictions, the ideas of the day, which, in their eyes, were not possessed of the merit of unmixed novelty, and added them, without noise, to the mass of the ancient doctrines. Others again, in despite of a fixed resolution not to depart from the opinions of Brown and Pinel, suffer daily to escape some tacit confession which they hardly dare acknowledge to themselves; another and a numerous body adopt the new principles with enthusiasm, and, in frequent instances, permit exaggerated impressions to acquire the force of prejudices in their medical education; while some, in fine, content themselves with cultivating morbid anatomy, without appearing, in this, to seek for the support of any theory, being either unsettled in their selection of that one which they are to adopt, or willing to conceal, under an industrious research for facts, their adherence to some one which has lost or is losing the popular favour."

We deem it unnecessary to make much comment on the above opinions. We do think that there is a decided preference due to the "eclectic," progressive, or *additive* class which is enumerated in the second place; and to which M. Boisseau evidently wishes to attach himself. As every physician, however, is an independent agent, moral and philosophical, and must conscientiously act upon his own convictions of truth, we conceive that we do enough by laying our materials before the reader for his private judgment.

The Nosographie Organique is a description, in detail, of the diseases of the different organs of the body, considered separately; and constitutes a *synthetic* study of pathology. We have received the first volume only; which treats of the diseases of the alimentary canal, beginning at the mouth and concluding with the rectum. As we have determined, for reasons given above, to begin our notice with the analytical work, the regular consideration of the present will very probably run beyond the limits of a review; in which case, it is our intention to take it up in our next number. In the meanwhile, we may occasionally use it to illustrate points which may occur in the consideration of the Pyrétologie.

After a short introduction, in which he defines excitability, superexcitement or irritation, and subexcitement or asthenia, the author proceeds to a brief historical sketch; from which we shall borrow no more than we have already done, with the exception of the remark that Hippocrates, and the other authors under his name, are not to be held accountable for the modern classifications and definitions of fevers. The words πῦρ and πυρετὸς, so frequently employed by him, mean by no means the abstract conceptions which in later times have been attached to the words "a fever." They are merely used to express a symptom, viz. the *heat*, which is so remarkable over the patient's body. Thus πυρετὸς ξυνεχὴς, usually translated "synocha," or inflammatory fever, means nothing but "*continued heat.*" Bilious fever is not expressed in any single phrase in these writings; but the flow of bilious matter is specially mentioned where it occurs: neither do they define mucous or adynamic fevers. The word αταξία is used by Hippocrates to express *irregularity* or confusion, and not, as M. Pinel has used it, after Selle, to characterize, in an adjective form, the class of "ataxic fevers." Typhus, τύφος, merely means *stupor*, and τυφώδης πυρετὸς signifies a *heat with stupor*. Λοιμὸς, the plague, is equally ambiguous, and means, as various authors have shown, any terrible and destructive epidemic. It may easily be shown that subsequent writers were a long time before they began to attach ideas of nosological entities to these various expressions, which continued merely to express symptoms.

To proceed with our analysis, the first variety of fever which comes under our consideration is synocha, or "common inflammatory fever," as it is called. As our most generally diffused ideas on these subjects are obtained from Cullen, Darwin, and Rush, we have thought it proper to note their opinions on the alleged local origin of this fever. By the first of these, all primary local disease is expressly excluded, in his very definition of the class of fevers. Dr Darwin, on the contrary, states that "a strong, full pulse, with inflammation of the coats of the arteries, constitutes this disease. It originates from some topical inflammation; which, if the fever is not subdued, terminates in suppuration." (II. 1. 2. 1.) Dr Rush, after defining this fever, adds, "these symptoms sometimes occur in the plague, the jail and yellow fever, and in the small pox; but they are the more common characteristics of pleurisy, gout, and rheumatism. They now and then occur in the influenza, the measles, and the puerperal fever." (Outlines. Class I. Art. 4.)

The necessary brevity of a review does not admit of our copying M. Boisseau's copious list of symptoms: they correspond tolerably well with our ideas on this subject. When local symptoms of violence are superadded to the general character of pyrexia, the fever is considered, says M. B., as being *complicated* with some local irritative disease; as apoplexy, encephalitis, angina, pleurisy, &c. &c: and when an increase of severity occurs which is not ascribed to these causes, the fever is said to be *converted* into one of a gastric, adynamic, cerebral, ataxic, or other similarly diversified form. We are, by this arrangement, deprived of the information to be derived from morbid anatomy; as the disease either terminates in health, or is said to be complicated with or converted into, something of a different character. Dr J. P. FRANK, however, has found the lining membrane of the arteries inflamed; from which the disease would appear, as considered by Dr DARWIN, to consist chiefly in an arteritis. At the present day, according to Dr B., every one is agreed that the origin of this disease is an *excitement*, an *inflammation*, an *angiotenia*, &c. terms which amount to nearly the same meaning, and which describe an increase of vital action; the question still remaining open, in what part is this excitement situated. Many writers insist that it extends throughout the whole animal economy; others confine it to the whole sanguineous system, to the arteries, to the heart, to the great blood vessels, to the capillaries, &c. M. BROUSSAIS refers it to the mucous membranes, particularly those of the primæ viæ; M. ALARD, to the subcutaneous cellular tissue.

Inflammatory fever, according to our author, may originate from inflammation of any part of the body; and, when the point of irritation is the eye, ear, larynx, pharynx, or any other part with whose inflammations we are more familiar, it loses the above name, and assumes that of ophthalmia, otitis, angina, &c. &c. In the greater number of cases, the origin of the disease is in some part of the alimentary canal, forming some one of the varieties of gastro-enteritis; while, in infancy, it is quite as frequently seated in the brain; and with females, in the uterus. At the period of puberty, it is most apt to be in the thorax; and with hard students, in the brain.

Various parts of the body are frequently affected at the same time; such as the skin, the cellular membrane, the muscles, the synovial membranes, &c.; nor does the author admit, as correct, the reasoning of M. BROUSSAIS, that, if this were the case, we should meet with the symptoms of erysipelas or some other cutaneous disease, of phlegmon, of rheumatism, or of gout. All these parts are capable, as has been demonstrated by that celebrated teacher with regard to the stomach and bowels, of milder and more obscure grades of irritation than those described by the nosologists; and the heat, soreness, and tumefaction, which are occasionally met with in them, and the red flush of the skin, are abundant grounds for conceding to such irritations the inflammatory character.

As this local irritation manifests itself, in many instances, by obscure and slight signs, it is necessary for the practitioner to study, with great attention, both its remote and exciting causes, the idiosyncrasy of the patient, and the symptoms, in order to discern the real source of the disease, amid so many sympathetic and conflicting appearances. The importance of this is particularly enhanced by the reflection that it is only in this way that we can reasonably hope to prevent, by a well aimed and sufficiently energetic treatment, the *complication* or *conversion* of this *fever* with or into some of the more distinct, because more violent forms of local phlegmasia.

The principal difference, then, between this and the prevailing doctrine, is, that we are to study, with more care, the *mild* and *predisposing* states of local irritation; to suspect them sooner, and meet them with more promptitude, in order to prevent their sudden and dangerous growth. Instead of the fever *producing* phlegmasiæ, the phlegmasiæ already exist, and cause the fever; and it is our duty, as physicians, to find them out. In particular, it is necessary to be acquainted with the labours of Dr Broussais, in relation to the more moderate forms of inflammatory action in the primæ viæ.

The *treatment* consists of rest, diet, the abstraction of blood, emollients, and those derivatives which do not produce febrile irritation. A single bleeding is generally sufficient, unless the lungs are affected. Leeches should then be applied. If the abdomen be the seat of the primary irritation, they should be placed over the diseased part; except in dysenteric irritation, when they are most serviceable in the anus. The bleeding of the leech bites should be encouraged: if it become excessive, it should be checked by styptics, or, these failing, by cauterisation, either with a sharp-pointed piece of lunar caustic, a mineral acid, or the point of a red hot wire.

We are bound in conscience here to note the fact, that leeches are capable of destroying life. In addition to sufficient evidence to be drawn from books, the writer of this has himself been witness, in more than a single instance, to the fact. The best method of suppressing the hemorrhagy, is said to be, to dip a straw in oil of vitriol, and let a drop run into the bite.

The importance of diet, strongly recommended by HIPPOCRATES, and also by modern surgeons, has received a new elucidation from the labours of M. BROUSSAIS in gastro-enteric irritations. It should be combined with the use of mucilaginous diluents; both in these cases, and in irritations of the bladder. *If there be no gastro-enteric irritation*, nitre may be advantageously employed. Warm foot baths, emollient fomentations, cool spongings, &c. should be used with a physician's judgment, but more frequently than is generally the case among us. These remedies are extremely serviceable; although it is not uncommon to speak slightingly of them, as old women's remedies. We are very sure that if they be such, the old women will, in some cases, be more successful than the practitioner who confines himself to the more valiant remedies of the shop.

All contra-irritants, or revulsives, which produce fever, such as sinapisms and blisters, are forbidden; nor can even the milder ones be used with advantage, if the skin be highly excited. Tepid or cool spongings are all that should then be applied to this irritable organ.

Bilious Fever.—Fièvre Gastrique is sometimes bilious and sometimes not. When its premonitory symptoms of the local kind are principally referrible to the stomach, the patient is affected with "embarras gastrique," which we will translate "foulness of the stomach;" if there be colic, borborygmata, costiveness, or a diarrhœa discharging bilious matters, it is an intestinal "embarras," or foulness; and these may be combined. If a chill occur, soon followed by high fever, thirst, anorexia, an increased bitterness in the mouth, pain in the forehead, a yellow skin, &c. &c. it is a bilious fever. If violent vomiting, hiccup, copious stools of undigested matter, followed by variously coloured liquids, with cramps, severe pain, and great shrinking of the surface of the body occur, it is a cholera.

No person dies, according to M. BOISSEAU, of a pure bilious fever, but only of its complications; so that we are here again deprived of the torch of anatomy. After cholera, the mucous membranes of the colon, duodenum, and stomach, are found thickened, with various points of a lively red, and others of a brownish-red, and sphacelated: frequently, also, the intestinal canal is much contracted—an appearance which, according to M. GEOFFROY, is demonstrative of inflammatory action. When the head is severely affected, the pain in the epigastrium diminishes, or perhaps ceases altogether; but we may observe, at the same time, that pressure on this point, although not productive of pain to the apprehension of the patient himself, will nevertheless produce that shrinking of the features, which occurs during real pain. If you press upon the epigastrium, and, at the same time, watch the face of the sufferer, you will perceive a contraction of the eyebrows, similar to frowning, which is renewed at every repetition. From this fact, the physiological physicians infer that there is a real nervous action produced, of a nature similar to that which occurs in the production of pain; but that the disorder of the brain renders it incapable of perceiving the sensation. By the caprice of artificial classification, we are prevented from being able to assign any morbid appearances as characteristic of bilious fever; as if death occur, the alterations found are referred to ataxic, adynamic, typhus, &c. terminations, or to inflammatory complications. Analogy to cholera, however, of which men are permitted to die, together with the great similarity of the exciting causes to those of cholera, and with the strong marks of irritation about the stomach, evidently points out the latter as the first scene of the morbid derangement.

In fact, bilious fever seems to have been singularly neglected in the labours of those who have supplied us with our most familiar definitions. Dr Rush, in his outlines, scarcely mentions it, as occasionally producing the symptoms of synochus. Dr Cullen cannot be properly said to mention it at all; or Dr Darwin. Dr Good makes it a sub-species of "enecia cauma," or inflammatory continued fever; and treats it with corresponding brevity. We are thus left, for the acquisition of clear ideas, to the descriptions of epidemics; undoubtedly a very excellent resource for practical purposes, but not at all a good one for definitions. Whatever efforts may be used to demonstrate the origin, in any particular manner, of bilious fever, can hardly be said to be in opposition to the established opinions; as it is nearly, if not quite impossible to ascertain on this point, what the established opinions are. This omission of so important a class of diseases, is at least singular; particularly in the authors most commonly used as text books over a country, where these complaints so frequently abound.

The *biliousness* is considered by our author, after professor Broussais, as the result of a sympathy extended from the primæ viæ to the liver; most probably along the course of the biliary ducts. Nothing is said, here, of the *torpor* of the liver, and the absence of bile, which are occasionally so alarming. When bilious symptoms are wanting, in a gastric fever, the case is so much simpler. The predisposing stage of bilious or other gastric fever, is considered by M. Boisseau as identical with dyspepsia or indigestion; and as generally arising from undigested or excessive aliments or other stimulation to the stomach. The *treatment* adapted to this stage, hence, becomes a matter of interest, not only as a prophylactic, but as throwing light on the character of the subsequent disease. He finds warm, diluting drinks the best and sufficient treatment; operating both by vomiting, without any increase of irritation, and by favouring the discharge of undigested matters and excessive bile by stool. M. Pinel recommends emetics as a remedy of general use. M. Broussais, as is well known, believes these to aggravate the disease in a great number of cases. Our author states the frequent occurrence of a greatly increased severity in the symptoms of the disease, after using emetics; and the opinions of many physicians of the school of Pinel, that the remedy unlocks the disease, and suffers it to develop itself. This Dr Boisseau believes to be not a development, but a real augmentation of the fever. The experience of physicians will decide for themselves: our own impression, however, distinctly is, that where the presence of actually offending materials, such as undigested food, spirituous liquor, or bile in considerable quantity, in the stomach, is distinctly proved, emetics of ipecacuanha have, in almost every case, been beneficial. We must in honesty, however, plead guilty to having repeatedly seen fever increase after the use of such remedies; although we have seldom used them. Enough is certainly proved to justify an earnest care to avoid all unnecessary irritation of the already disordered stomach in procuring the desirable evacuation.

If there be redness of the edges and point of the tongue, whiteness of its central portion, loss of appetite, acidity, decided repugnance for wine and spirits, a sense of weight at the epigastrium, and perhaps a slight weakness of the legs, the case is a light or mild gastritis (légère). The treatment is—entire abstinence, the use of mucilaginous or acidulous drinks sweetened, or even, if the patient demand it, of pure cold water, and moderate exercise in the open air. The emission of blood can generally be dispensed with; but if a greater severity exist, a dozen (large) leeches are to be applied over the epigastrium. Up to this period, says Dr B., we could defy the nosologist to decide whether the case be a proper inflammatory fever, or a gastric one. For the physiological physician, the problem is entirely solved: he knows that he is dealing with a gastritis; and that nothing should be neglected which may prevent it from increasing.

When a fever is completely formed, with vomiting, and other marks of gastric fever, but without bilious symptoms, twelve, fifteen, or twenty (large) leeches are to be applied to the epigastrium; and the bleeding from their bites promoted till such an amelioration is produced as authorizes a hope of cure: the process to be repeated in proportion to the intensity of the disease and the strength of the patient. Dr B. prefers the keeping up a very long continued hemorrhage from a small number of leeches, to frequent repetitions, a preference which, in the use of American leeches, we of course cannot exercise. Leeches, with him, are occasionally liable to produce inflammation and abscesses of the cellular membrane; and this is an additional argument for prolonging the hemorrhage from their bites.

We read so often in mere general terms of the "boissons mucilagineuses, acidulées," that it may be well enough to particularize those recommended in this work; and we can say, from experience, that their choice is by no means a matter of indifference. The acidulation should be weak; and this should be omitted as soon as the patient perceives a sensation of *pinching* in his stomach. Lemonade is generally useful. Orangeade, prepared in a similar manner with orange juice, often answers better. Gooseberry water may be used for change, or as a substitute; but it does not agree so well with the majority of stomachs. Decoctions of cherries, the "pomme de reinette," and other acidulous fruits, and of sorrel, are valuable; and whey is often very advantageous. Mucilaginous drinks are sometimes to be suspended, from producing a sense of weight in the stomach, and a clamminess in the mouth. They are such as gum arabic water, light decoctions of marsh-mallows, of "chiendent," [Erythronium dens canis ?] barley water, and toast water, observing that the two latter should not be heavily loaded with fecula. *Pure water!* is, in a great number of cases, the best of all possible drinks. For the possession of this luxury, so often refused to the parched and broiling fever patient from fears which had their origin in the doctrine of *coction*, our author cites the at least respectable authorities of HIPPOCRATES, GALEN and CELSUS! We have, for many years, been in the habit of permitting moderately cool water in these cases, and are glad of the occasion to furnish the sufferer with this additional defence of the propriety of what he craves. Drinks should be taken in small quantities and often; an observation of peculiar importance during a tendency to vomit. The patient should be urged not to take a large quantity, but to drink with sufficient frequency. Sometimes but a single spoonful is advantageously given, and that at long intervals. Great pains should always be taken to ascertain the articles, and the manner of administering them, which agree best with the inflamed viscus in each particular case. The *temperature* of the drink may generally be left to the feelings and wishes of the patient himself, who is often the best judge. Exceptions are found in the use of cold water to check, and warm water to promote vomiting, of warm drinks during chills, of hot fluids in certain particular cases, from idiosyncrasy or otherwise, &c.

The interdiction of articles of heavy digestion is, as our author very fairly remarks, somewhat unnecessary, as the patient generally can be hardly prevailed upon to swallow them; but a not unfrequent abuse, met with commonly enough in our country, is the great anxiety of nurses to induce patients, often against their own inclination, to take *broth*. Dr BOISSEAU unites with professor BROUSSAIS, in carrying this objection so far as to say, that it is quite possible for the lightest broth to produce fatal relapses. Strong disapprobation of this error ought, if possible, to be impressed on the popular mind; as the difficulty is extremely enhanced by the idea, derived from the former opinions of physicians, that it is necessary to give *strength* to the patient.—Demulcent emulsions are only admissible, when the irritability of the stomach is very moderate; and the same may be said of Seltzer water, which should be weakly charged with gas.

Warm, emollient *fomentations*, or light cataplasms, are placed over the abdomen, and have the advantage, in addition to their direct effect, of promoting the bleeding from the leech bites.

Costiveness is not considered a very important complication of gastric fever; and this forms one of the strongest points of dissimilitude between the French and English practice. Dr B. acknowledges, however, that it does produce headach and febrile heat; but conceives that the only means which should be employed for its relief, are injections of water, either pure, or mixed with mucilage, vegetable acids, oil, or honey. We do not stop here to discuss this much debated question, which has almost become national. The prohibition of purgatives is well understood to be on the ground that they create additional irritation in the inflamed and sensitive viscera; and it is asserted that experience has pretty uniformly shown disadvantageous consequences from their use. We presume our readers will hardly be willing to subscribe to this last statement; and yet we think it will be found to be *sometimes* correct. Without imitating the practitioner in the tale, who noted down that "salt herring cures an Englishman in a fever, but kills a Frenchman," we will ask, is it quite impossible that there may be a difference in the idiosyncrasies of nations? We have heard it said that the Americans eat more than the French. Can this render it probable that purgatives would be both more useful and better tolerated with the former?

The headach commonly ceases when the other symptoms are relieved, as it is originally only sympathetic; but, if it be severe, it indicates hot pediluvia, including those with mustard, cold applications to the forehead, and leeches to the temples.

It is hardly necessary to add, that *vomits* are decidedly contraindicated.

The diagnostic symptoms which M. Boisseau uses, to separate bilious fever from the form of which we have just been speaking, are, pain in the right hypochondrium, a thick yellow coating over the middle space of the tongue, which is dry, with its point and edges as in the former case, a bitter taste in the mouth, a more strongly marked repugnance for food, especially rich substances, the vomiting of bilious matter, coloured yellow or green, &c. extreme hardness of the pulse, greater acrimony in the heat of the surface, yellow skin and eyes, a more acute pain in the forehead, temples, and base of the brain, and a thickness and deep yellow colour of the urine. Some of these symptoms imply merely a greater extent or violence of the disease. The treatment implies a commencement by the means already enumerated, under which the hepatic symptoms will frequently yield; but should they prove more refractory, leeches must be applied to the right hypochondrium, or to the anus, and warm baths must be employed. *Calomel*, we see, is omitted.

The author next describes the circumstances under which he thinks that an emetic *may* be prescribed. They amount to this, that, in the preliminary stage of *embarras gastrique*, provided there are no symptoms of fever, nor soreness at the epigastrium, and particularly when there are no signs of hepatitis, if the symptoms do not yield to the previously described treatment, we can give an emetic if we choose. Whenever there is heat of the skin, acceleration of the pulse, and unusual redness of the tongue, it is formally contraindicated. Of course, the use of vomits during *fevers* is decidedly objected to under any circumstances. In this place follows a defence of the well-known French opinion, that vomits should never be given till after a preparation by diluting drinks; an opinion which often seems absurd to those unaccustomed to it. Dr B. maintains it on the principles of the physiological medicine; and admits the use of emetics in the decline, or, to speak more correctly, after the termination of fevers, we can hardly tell with what object. His rules are nearly the same thing with a mere prohibition; but they may be used in those affected with transient bilious diarrhœas, in those accustomed to take vomits annually, in stomachs simply overloaded, and not, at the same time, unduly stimulated, in persons corpulent, pale, with soft flesh, and of little excitability, in cold and wet countries, and in high northern latitudes.

M. Boisseau states that, by the preceding treatment, gastric fevers are entirely removed, sometimes in a single day, ordinarily in a very few, and most generally in a week. They are never seen to be prolonged for two or three weeks, and still less for a month or six weeks, "as so often happens when vomits are insisted on."

Hepatic symptoms sometimes continue for a few days, after the gastric derangement has ceased, and without being accompanied with any fever. In these circumstances, a saline purge may be given with advantage; but diets and drinks will commonly suffice.

Our author's reflections on *cholera* will not detain us. It is inflammatory; but he does not possess personal experience, nor is there in his reach a sufficient number of well ascertained and well described cases.

In the course of gastric fever, there occur occasionally inflammatory affections of the bronchiæ, the parenchyma of the lungs, the pleura, the kidneys, or the bladder. In these cases, leeches should be applied, and sometimes blood taken from the arm; as it is unsafe to wait for the spontaneous subsidence of these complications, and they may do irreparable injury before the termination of the disease.

Inflammation of the peritoneum sometimes occurs in these fevers; but Dr B. postpones the consideration of it, as such cases are classed among adynamic, or our typhoid, fevers.

The *period* at which patients may be permitted to return to their ordinary food, is a point of great importance. " Broth should never be allowed till the skin has lost the *calor mordens*, and its temperature is but very little elevated above the ordinary height; and it is better even to wait for its entire return to the natural standard, and until the pulse has ceased to be hard and frequent, and the tongue to be red at its point and edges." It is beneficial to dilute the broth with vegetable decoctions. Afterwards we may add a light decoction of rice, barley, bread, or vermicelli, and subsequently soup. The return to ordinary food should be gradual. The patient should for a long time refrain from wine; especially when unmixed with water. Previously to using meats, he should be kept on stewed and preserved fruits, and on legumes. If costive, he should employ enemata, and a laxative diet: veal broth, decoction of sorrel, tamarinds or prunes, and whey, are recommended. Purgatives should never be used but in two cases; either where the appetite diminishes, and the tongue becomes foul, without fever, or, secondly, where the patient is accustomed to their prophylactic use at stated periods. Purgatives are liable to bring on relapses; and these occur still more frequently from overloading the stomach. Hence a continuance of the same regimen is inferred to be the best protection during convalescence: to which may be added moderate exercise, and, in young persons, the cold bath. " The administration of tonic medicines, far from shortening a convalescence after bilious fever, may suffice to render it *interminable*."

We have been thus long and minute upon the subject of bilious fever, because it is in its nature elementary; furnishing a fair specimen of the nature and treatment, according to the " physiological school," of *gastro-enteritis*. We shall now be the better prepared to enter upon the more complicated forms. We propose to ourselves the consideration in a future number, of those forms of fever generally known among us by the appellation of typhus.

Article IX.—*Recherches Anatomiques et Physiologiques sur la Structure Intime des Animaux et des Vegetaux, et sur leur Motilité. Par* M. H. **Dutrochet**, *Docteur en Médecine, Correspondant de l'Institut de France dans l'Académie Royale des Sciences, Membre Associé de l'Academie Royale de Médecine, des Sociétés Philomathique, Linnéenne, et Medicale, d'Emulation de Paris, des Academies de Rouen, de Lyon, de Toulouse, &c. Avec deux Planches. A Paris*, 1824.

Anatomical and Physiological Inquiries into the Intimate Structure and Motility of Animals and Vegetables. By M. H. **Dutrochet**, M.D. *&c. &c. With two Plates. Paris*, 1824.

L'Agent Immediat du Mouvement Vital, devoilé dans sa Nature et dans son Mode d'Action, chez les Vegetaux et chez les Animaux. Par M. H. **Dutrochet**, *Correspondant de l'Institut dans l'Académie Royale des Sciences, Membre Associé de l'Academie Royale de Médecine, &c. Nil tam difficile est quin quærendo investigari possit. (Terence, Heautontumorumenos.) A Paris*, 1826.

The Immediate Agent of Vital Movement, displayed in its Nature and Mode of Action, both in Vegetables and Animals. By M. H. **Dutrochet**, M.D. *&c. &c. Paris*, 1826.

It has been a matter with us of frequent regret that we have not been able to furnish our readers at an earlier period with an analysis of the two works, the titles of which are given above. But various reasons of a nature already alluded to in our last number have interposed to prevent the execution of our wishes in this particular.

Doctor **Dutrochet** has always evinced a partiality for the study of minute structure, and inquiries into what may be called the mooted points of physiology. We wish there were fewer of them. His inaugural dissertation in 1806 was on a New Theory of the Voice. Since then he has written essays on the following subjects; Habit and Sympathies; a new Theory of Harmony; Inquiries into the Rotifera; into the Envelopes of the Fœtus; and into the Metamorphosis of the Alimentary Canal of Insects; History of the Eggs of Birds before they are laid; Observations on the Structure and Regeneration of Feathers, with some General Considerations on the Composition of the Skin in Vertebral Animals; Inquiries into the Growth and Reproduction of Vegetables; and Observations on Osteogeny; together with some other questions in natural history. Most of these have been inserted in some scientific journal, such as the *Journal de Physique*, and *Memoires du Museum d'Histoire Naturelle.*

The author of these works, acting up to his belief that all the classes of organized beings ought to be carefully examined and compared with each other, since the same general laws govern all, has devoted much time and labour to vegetable physiology, with a view to its application to animal physiology. His success has not we think been small in this part of his inquiries. But that our readers may be enabled to judge for themselves we shall begin, forthwith, our duty of analysts. In the introduction to the first work, on the Intimate Structure of Animals and Vegetables, Dr DUTROCHET, after some remarks on sensibility and on its being applied to express the consciousness of certain impressions made on a living body, and of course on its being an objectionable term in physiology, conveying as it does a purely moral intellectual idea, proposes the suppression of the word. Of the precise nature of the movement, produced by external agents which modify the senses on which they act, we are ignorant. He proposes affixing to this phenomenon, produced in the senses by external agents and transmitted by the nerves, the term *nervimotion*, and to the vital property in virtue of which it takes place, the term of *nervimotility.* Those external agents capable of producing nervimotion are called by him *nervimotor agents.* Nervimotion is purely a physical phenomenon, constantly preceding the moral one of sensation, but not necessarily followed by it. Hence our internal organs possess nervimotility, they experience nervimotion; but sensation does not result as is the case with our external organs. Life, in regard to its physical relations, is nothing else but movement; death is the cessation of this movement. Living beings exhibit different faculties of movement, the chief of which is *nervimotility* or the power of experiencing certain modifications or changes in their existence, by means of particular agents from without, *nervimotors.* This first movement, which is invisible, is the source of the visible one, executed by living organs. The faculty of executing these movements, by which a displacement of parts is operated, may receive the name of *locomotility:* it presents two movements of an opposite nature, *contraction* and *turgescence.* All these powers of movement are referrible to a single general power. which the author designates by the name of vital *motility:* it is life itself. Vital motility exhibits to us in all living beings the same principal vital phenomena. Throughout all we find *nervimotility*, and consequently *nervimotion*, under the influence of *nervimotor* agents: in all there is *locomotility* or the faculty of changing the position of the parts. Vegetables display, as well as animals, these two faculties of movement, but they are much less energetic in the former, and much less distinctly developed. The laws presiding over the vital motility of animals are of very difficult study, on account of the extreme complication of causes, as well internal as external, which exert an influence over the faculty. In vegetables, on the other hand, the study is greatly simplified, and it is probably in them alone that we can hope for a solution of the principal problems of the science of life.

After these premises, Dr Dutrochet proceeds to inquire into the anatomy of vegetables, and especially into that of the sensitive plant *(mimosa pudica, L.)* He admits that the usual means of observation and discovery in vegetable anatomy have received every variety and latitude of application possible. That he has been able to go farther than his predecessors in this department, is owing to the novel process to which he had recourse for separating the different parts of the internal organs of vegetables. He succeeded in overcoming their adhesion, without destruction of tissue or alteration of intimate structure. After many trials he found the following to be the most advantageous procedure. He put a portion of a vegetable which he desired to study, into a small vial filled with nitric acid, and then plunged this into boiling water. By this operation, the component parts of the vegetable tissue lose their aggregation and become transparent, and the study of them is singularly facilitated. The tracheæ and other vessels are at the same time filled with an aeriform fluid, giving them a peculiar aspect under the microscope, and furnishing us with a new means of observation. It must of course be understood, that this operation is not to be carried too far, otherwise the vegetable tissue would be quite disorganized. The less prolonged the ebullition the better: in general we ought not to wait until the vegetable has become quite transparent and divided spontaneously.

It was the desire of knowing the anatomy of the sensitive plant which led the author to make these researches, and it is the anatomy of this plant which will serve for his text, to be illustrated when necessary, by considerations drawn from the structure of other vegetables. He begins by the medulla or pith. It is, like the corresponding part in all vegetables, entirely composed of cellular tissue. The cells present an hexagonal figure, regular in some places, less so in others, and generally arranged in longitudinal series. Not satisfied with the comparison, made by GREW and adopted by MIRBEL, of this tissue to the froth of a fermenting liquor, Dr DUTROCHET examined it after immersion in nitric acid and subsequent ebullition, and found that all the cells separated from one another and presented so many complete vesicles, which preserved the form precedingly given to them by the mutual compression of their sides. Thus, wherever two cells touched each other, the intervening partition consisted of a double membrane.

In the medulla of the sensitive plant, each cell has several rounded corpuscles, opake on their sides and transparent in the centre, an appearance presented by all transparent spherical bodies. They were rendered entirely opake by heated nitric acid, while the cells became quite transparent. Having placed some of them on a piece of glass and added a large drop of a warm solution of caustic potass, holding the whole over a spirit lamp, he found the corpuscles regain their primitive appearance. The next question was, what office did these corpuscles, concrescible by acids and soluble in alkalies, perform. The answer must be sought by comparing them with the agglomerated globular corpuscles of animals, which microscopical observation has shown to be analogous to those in the organic tissue of vegetables. But as nothing can be inferred from the figure of these globular corpuscles, which entirely compose the organs of animals, we must have recourse to differences in their chemical nature. Accordingly we find that those of which the muscles are constituted are soluble in acids, whilst those entering into the composition of the nervous system are insoluble in acids, but soluble in alkalies. The latter are the characters of the globular corpuscles of vegetables, and would lead to the conclusion, that these bodies are nervous organs, or rather the scattered elements of a nervous system. This belief is strengthened by a comparison of the structure under consideration with the nervous system of the mollusca gasteropodes, the medullary substance of the brain of which consists of agglomerated globular cells, on the sides of which are seen a great number of globular or ovoid corpuscles, represented in figure 20 of the plate, attached to the work. These remarkable analogies, taken in connexion with the singular phenomena presented by what are called the *irritable* vegetables, do not, says Dr DUTROCHET, permit us to doubt, that there is something similar in them to the functions performed by the nervous system in animals, and tend to prove that there is, if not a nervous system, at least some of the elements of one in plants. Influenced by these considerations, he designates the globular corpuscles of a concrescible nature, situated on the sides of the cells of vegetables, by the term *nervous corpuscles*. This he uses in place of the periphrase of a *microscopical globular cell, filled with nervous matter*.

We have neither time nor room to give the author's criticisms on MIRBEL and LINK's opinions respecting the arrangement of the trachea in the medullary sheath. It is sufficient for us to repeat, after him, that these parts are studded externally with nervous corpuscles—an arrangement very evident on the stalks of the *solanum tuberosum* and *cucurbita pepo*. Alongside of the tracheæ are vessels double the size of these latter, and called by MIRBEL porous tubes or false tracheæ, but which Dr D. discovered to be corpusculiferous vessels; that is, tubes lined on the inside with numerous nervous corpuscles. (Fig. 8. 9. 10. 11. 12.) He thinks that these latter are the ascending sap vessels of the plant: they are found not only in the medullary sheath, but in the whole central system of the plant, and, in ligneous vegetables, are especially seen in the spaces between the annual layers of the wood. They abound in the wood of the vine, and it would seem to be by their orifices that the sap flows out so abundantly, when the branches of this plant are cut in the spring. No small force presides over this movement of ascension, which, be it now said *en passant*, cannot be the effect of capillary attraction; since the sap does not rise in the dead branches, which are contiguous to and continuous with the living part of the plant, although the former retain their capillarity.

The functions of the tracheæ have been the subject of many discussions among naturalists. Some considering them as analogous to the tracheæ of insects have styled them respiratory organs; others affirm that these tubes never contain air but sap, an opinion which Dr DUTROCHET thinks the correct one. They are, he says, assuredly the conductors of a diaphanous liquid, and never, in their natural state, do we find a single globule of air in them. They may indeed by boiling in nitric acid be, in common with all the other tubes of the plant, filled with air; but then their appearance is peculiar, and very different from that presented in the living vegetables. They have a necessary and immediate connexion with the functions of the leaves, in which and the medullary sheath only they are found—parts these in the young branches in intimate relation to each other. With the offices performed by the leaves, we are not yet perfectly acquainted, notwithstanding the investigations of INGENHOUZ and SENNEBIER, and above all the more extended labours of THEODORE DE SAUSSURE. It is, however, a point well ascertained, that light exerts a special *vivifying* action on the leaves, either by itself, or by causing certain chemical changes in the fluids of their vessels. The tracheæ would seem then to serve the purpose of transmitting to the body of the plant the fluid thus modified and vivified in the leaves by the action of light.

The part to be studied next is the ligneous, which covers the medullary sheath. This is possessed by the sensitive plant, though it be herbaceous. The wood is, we are told by the author, composed chiefly of tubes, cylindrical in their middle, and terminating in conical points (fig. 13.) These fusiform tubes he calls closters, the sides of which touch each other at the centre, while the intervals between the points are filled up by the ends of other closters. Several of these are separated into two equal parts by transverse partitions. The solidity of the vegetable tissue more particularly depends on these closters, which are the reservoirs of a juice capable of concretion and of acquiring a great degree of hardness. It is by this means that the alburnum is converted into wood. The colouring matter of different kinds of wood depends not on the closters themselves, but on the concrete juice with which they are filled. On this last depends likewise the relative hardness of the ligneous portion, for the closter has the same dimensions in the box tree (*buxus sempervirens*) and the poplar, that is to say, in the hardest and softest wood.

The closters seem to be, in the alburnum, or sap wood of recent formation, the reservoirs of elaborated sap, specially destined to furnish matter for the nutrition of the plant in its diameter, and which, transmitted from closter to closter, by a descending movement, supplies the roots with materials for their growth. The author thinks that it is by means of this admixture of the elaborated with the ascending sap, that the buds receive matter for their growth, and that the former furnishes to the proper vessels materials for their secretions. It is by a similar diffusion of an elaborated juice that nutrition and secretion are performed in insects.

The fasciculi of closters are blended in the sensitive plant with a cellular tissue, which can be mechanically divided into longitudinal filaments consisting of a series of cells (fig. 15.) To this arrangement the author gives the name of *articulated cellular tissue*, which, continued in a longitudinal direction, is observed to become tubes. This cellular tissue resembles that of the medulla or pith, and like the latter is covered with nervous corpuscles irregularly arranged. It is the generating organ of the medullary rays in the ligneous and herbaceous plants.

The cortical system of the sensitive plant is composed of closters larger than those which are found in the central system, but resembling them in being destitute of nervous corpuscles. Both contain a liquid, concrescible in cold and soluble in hot nitric acid. Reasoning from this perfect identity of structure and chemical composition, the author thinks himself justifiable in giving to the pith the name of *central medulla*, and to the parenchyma of the bark that of *cortical medulla.*

The leaves of the sensitive plant are borne on a long petiole, at the termination of which is a swelled portion which Dr DUTROCHET calls *bourrelet* (fig. 18.) Similar enlargements, but proportionately less, are seen on the insertion of the pinnules into the petiole, and at the junction of the leaflet with the pinnule. In these enlargements exists the moving principle of the plant, as we shall see hereafter.

An investigation of the structure of the joint, or the *bourrelet*, formed by the union of the petiole with the branch of the plant, shows it to be chiefly composed of cortical parenchyma, which is itself made up of globular and diaphanous cells, the sides of which are covered with nervous corpuscles. The cells are precisely similar to those already noticed in the medulla and cortical parenchyma. In addition to these parts, we discover elongated closters, which form as it were the bark of the petiole, and in its interior are articulated and corpusculiferous cellular tissue, and large corpusculiferous tubes. In the centre of the petiole are spiral tracheæ.

The leaflets contain an immense quantity of nervous corpuscles, discoverable by the means already indicated: they are very minute, in groups around the ribs, or rather the vessels traversing the leaflets.

The root of the sensitive plant exhibits, in its central system, closters blended with large tubes, precisely similar in their form, size and position to the corpusculiferous ones of the stalk; but they have not nervous corpuscles on their sides; although we may presume that their absence is apparent rather than real, and may depend on their extreme minuteness. The articulated cellular tissue is arranged in medullary concentric rays in the large roots, and in longitudinal filaments in the radicles: its nervous corpuscles are very transparent and are not rendered opake by nitric acid. It is known that there is neither medulla, nor medullary sheath, nor tracheæ in the roots. The cortical system of the root does not differ essentially from the same system in the stalk, as regards anatomical arrangement. Dr Dutrochet could not, however, discover that the cells of its parenchyma contained a fluid concrescible by acids.

If a young stalk of the sensitive plant, or the joint of the petiole of one of its leaves, be cut, we see immediately escape a drop of a diaphanous liquid, which, examined by the microscope, seems to be composed of a great number of transparent globules. These, when a drop of nitric acid is applied, are promptly rendered opake, and are restored to their fluidity by caustic potass.

The different hollow organs which we have pointed out in the vegetable tissue, viz. cells, tracheæ, membranous tubes and closters, have no other relation to each other than that of contiguity. There is no direct communication between their cavities; nor can their contained fluids be transmitted from one to the other, but by the pores of their sides. But in admitting this, the author does not think Mirbel's opinion of there being visible pores well founded. As each organ has its own membrane, the fluids must traverse the two contiguous sides, in order to pass from one hollow organ to another.

We have thought it proper to premise thus much of the anatomy of vegetables, as well that the reader may better understand the physiological part of the works before us, as for the purpose of future reference, comparison, and illustration.

The second section consists of *observations on the movements of the sensitive plant.* These, as the author very justly remarks, have been a fruitful theme for inquiries and speculations with the learned, and yet we are still in ignorance of the organic tissue, which evinces the faculty or property called *vegetable irritability*, a faculty not yet distinguished from *sensibility* by physiologists: or to adopt his own phraseology, we are ignorant, whether *nervimotility* and *locomotility* have a separate existence in the sensitive plant. Anatomy can alone guide our inquiries in these questions. By it, as set forth in the preceding pages, we have learned that the sensitive plant possesses a well developed nervous apparatus, especially evident in the leaves and the enlargements (*bourrelets*) of their articulations. Is this nervous apparatus the seat of the locomotility of the plant, as it is of its nervimotility ?

We find at once an obvious difference in the movements of displacement, presented in the parts of vegetables, from that met with in the limbs of articulated animals. The former have no mobile articulations like the latter: their locomotion is always brought about by means of an inflexion of parts endowed with great pliancy and softness: their organs of motion are in the very spot in which the flexion is produced. It is here that the organic tissue undergoes an internal movement, which determines the flexion or straightening of the part. In observing the movements of displacement in the sensitive plant, we see that on the slightest touch or commotion, its leaves close with great rapidity; a phenomenon which takes place as follows. The leaflets incline by pairs to each other, so that their superior surfaces are in contact, and they approach their common axis or the pinnule; the pinnules bend by an approximation to their common axis, which is the petiole, at the summit of which they are inserted by pairs; the petiole next bends by being averted from the stalk in which it is implanted. This movement of removal on the part of the petiole is so great, that it inclines towards the earth in approaching that portion of the stalk below its insertion; hence its movement is in an inverse direction to that of the pinnules and leaflets. All these movements are performed by means of the flexion of certain knots or *bourrelets* at the base of these mobile parts. The *bourrelet* of the petiole being the only one large enough for examination, the inquiries of the author were chiefly directed to ascertain what portion of it is the organ of motion, the central fasciculi of tubes, or the delicate cellular tissue containing globular cells and nervous corpuscles.

The whole cortical parenchyma of the *bourrelet* was removed by gently scratching it with a sharp penknife, so that the central fasciculus of tubes was completely exposed. The leaf was not destroyed by this operation; but the leaflets remained some days without being unfolded or expanded, and the petiole lost the power of moving. The two movements of flexion and straightening or extension seemed to indicate the connexion between these functions and the upper and superior parts of the knotted joint or *bourrelet*. Accordingly, Dr DUTROCHET found, that a removal of the parenchyma of the superior part was followed by inability of the petiole to incline towards the earth, that is to perform the movement of flexion, whilst destruction of the inferior portion prevented the extension or straightening of the petiole. This latter is not, however, to be regarded as a passive process, or the mere resumption by the petiole of its former position, after the power of flexion is quiescent. The difference is merely in the direction, the action on one side of the joint inclining the leaf to the earth, but on the other directing it to the heavens. For the vigorous performance of either an abundant supply of sap is required; between which and the motility of the leaves there is a very intimate relation. The author discovered also that the elastic force by which the above movements of *incurvation* are performed is evinced in slices, however minute, of the *bourrelet*. This phenomenon was more particularly observable, when they were immersed in water, and then according as the slices were taken from the upper or lower surface, would be the direction of their curvature. If immersed in other fluids such as an acid or alkaline solution, the curving movements were immediately arrested, showing that *elastic incurvation*, either in parts or in the whole of the joint of the leaf, is a vital phenomenon.

But we must not consider the *bourrelet*, or joint, as composed of merely two sets of antagonizing springs, inferior and superior. A lateral flexion may be produced by deranging the natural direction of the leaves of the plant to the light, and in fine, the petiole may move in every direction. We find, accordingly, that while the leaflets and pinnules move in the sense of *adduction*, by inclining towards the superior part of their common axis, the petiole, on the contrary, is moved in the sense of *abduction* by withdrawing from the superior part of the stalk in which it is inserted.

This vital incurvation is seen not only in the sensitive plant, but in the *hedysarum gyrans*, the oscillatory incurvation of which is exercised in directions alternately opposed to each other. The *cactus opuntia* and *berberis vulgaris* also exhibit it in their stamina, as does the *dionæa muscipula* in its leaves. Besides the oscillatory there is likewise a *fixed incurvation* much more common than the first: an example is presented in the ovary of the balsam, the valves of which at the epoch of maturity are separated from each other and rolled into a spiral form. Their first mutual adherence was in virtue of their elastic power, or tendency to incurvation.

The next point to be determined is, whether vegetable incurvation is the result of nervous action brought into play by external agents. The author thinks he can demonstrate the affirmative very clearly. Thus when a stimulus is applied to a particular part of the sensitive plant, remote from the articulation or *bourrelet*, and produces motion in this latter, we may infer that nervimotility and locomotility are distinct properties. In other words, when the sun's rays are directed through a lens on a leaflet, and which thereby promptly approaches its fellow, and the adjoining leaflets in gradual succession downwards meet each other, to be followed in similar movements by the mutual approximation of the pinnules, and the flexion of the petiole, we cannot doubt that there exists an invisible movement, a vital phenomenon anterior to locomotion, and consequent on the application of an external cause. This phenomenon is *nervimotion*, invisible of itself and only appreciable by its effects. Whilst locomotility is circumscribed in the articulations or *bourrelets* of the plant, nervimotility on the contrary is diffused throughout all parts of it; an inference which, independently of the evidence afforded by the vital phenomena of vegetables, we should be inclined to draw, from the fact of the nervous organs or the elements of a nervous system being found in every part of the plant. Hence, if a burning glass be directed to the flowers of the sensitive plant, though there ensue no motion in them nor in their long common peduncle, yet nervimotion is produced, for we see, after the lapse of a few seconds, the leaves of the stalk bend towards each other. The same effect follows the application of heat to the flower bud or to the bark of the stalk. When a leaf is completely folded so that there is no farther appreciable motion, it is still susceptible of nervimotion, since the ustion of its leaflets provokes the folding of the other leaves of the stalk.

Here we are naturally met by the question, whether, as nervimotility belongs to all parts of the plant, it is the attribute of special organs or is possessed by all the internal ones. It is not necessary for us to give the details of the author's experiments on this point. He found that the removal of a portion of the bark did not prevent the transmission of nervimotion to the leaves beneath, nor was this process materially affected by the destruction of the pith, nor by the simultaneous removal of the bark and pith. The communication between the upper and lower parts of the plant by means of the ligneous tissue was sufficient for the phenomenon of nervimotion to take place. In the converse experiments of removing all but the bark, or the medulla, respectively, it was discovered that neither of these parts was endowed with nervimotility, or the power of nervimotion. Still farther, the cellular tissue of the articulation or *bourrelet*, filled with nervous corpuscles, was found to be incompetent to transmit stimuli or to experience nervimotion: hence this invisible movement is restricted to the ligneous portion of the plant. Of the organs or vessels in this latter, the author found that the sap vessels, or the corpusculiferous tubes, were the only ones capable of performing this function; and in these, the transmission is accomplished, not by the nervous corpuscles, with which they are so fully studded, but by the medium of the sap itself. In a word, he finds the transmission of the sap united inseparably with the transmission of *nervimotion*; an inference which we should hardly have drawn from the premises laid down by the author. He attempts, indeed, to qualify it by saying, that although the nervous corpuscles are strangers to its transmission, they are still the generating organs of this power.

Nervimotility belongs to the roots as well as to the stalk of the sensitive plant; a circumstance evidenced by an experiment of the celebrated botanist DESFONTAINES. If the roots be sprinkled with sulphuric acid, we soon see the leaves of the stem folded on each other, those nearest the root undergoing this movement the first.

The transmission of the nervous power, or nervimotion, is brought about with some slowness in the sensitive plant. It is quicker, however, in the pinnules and petioles than in the articulations of the stalk. Temperature was not productive of any marked effect on this phenomenon, except the cold was considerable: when the mercury in the thermometer fell to 48 degrees F., ustion of the plant on such occasions failed to produce any effect. Solar light exerts a most remarkable influence over the motility of the sensitive plant, a fact hitherto suffered to pass unnoticed by naturalists. When kept in a dark place, even though the temperature be elevated, its leaves are soon folded. But this diminution of motive power was not accompanied by any delay of nervimotion, only the extent of propagation was less than common. The higher the temperature of the air, the sooner and more completely did the plant lose its motility when removed from the light. Hence, if exposed to a heat of from 81 degrees to 83 degrees F., its motive power was entirely destroyed, in a period of four days and a half of obscuration; whereas, ten days were required of similar exclusion from light to produce the same effect in a temperature of 63 degrees to 77 degrees F. Exposure of the plant to the sun restores with great promptness its motility; the light of day has the same effect but in a slower manner. A few hours of the former agency are equivalent to as many days of the latter.

It is no forced induction then to admit, that light exerts over vegetables an influence similar to that displayed by respiratory oxygenation on animals. *Insolation* is to the former what *oxygenation* is to the latter; and *etiolation* is for vegetables a condition very analogous to what *asphyxia* is for animals. Still more, we learn from Dr Edwards's experiments, detailed in our last number, that asphyxia is much more rapid in warm than in cold blooded animals; and that in these latter it may be accelerated or retarded by augmenting or diminishing the temperature of the medium in which they are immersed. Now we have just seen that the sensitive plant undergoes in its motility similar modifications from external temperature.

Our notice of the third and fourth sections of the work will be very brief, as we consider them but episodes in the main argument. The author in them treats *of the peculiar directions affected by the different parts of vegetables*, and *of the influence of rotatory movement on these directions*. He has been, it seems to us, more successful in showing the fallacy of the hitherto received explanations of these points, than in carrying us farther back in the chain of causation. The ascending direction of the stalks and trunks of vegetables and trees, and the descending one of their roots, are vexed questions in vegetable physiology. Some have thought that the radicles of a plant were necessarily directed to or attracted by a humid soil, and that the plumula and stem had a constant tendency to the atmosphere and to light: but Dr Dutrochet has satisfied us, independently of the experiments of others, that this is not the fact. The embryo of misletoe seed directs constantly its radicle towards the centre of the branch of any tree on which it is fixed, or even of glass or metallic balls. The inference is, that the radicles of *terrestrial* plants are obedient to the attraction of gravitation; whilst the radicle of the misletoe or other parasites follows the particular attraction of the body on which it is placed. It may be added that all parts of a vegetable have a tendency to affect a direction perpendicular to their surface of im-plantation or insertion. Branches from the main trunk or stem, and roots around the central one, take a direction compounded of their tendency to grow in a line perpendicular to these parts, and of their other tendency to follow the general attraction by which the root is carried downwards, and the stem, stalk or trunk upwards. Hence roots and branches make an angle with the vertical axis of the vegetable of varying latitude.

These movements by which the stem and root of plants take particular directions are, in the author's opinion, *spontaneous*, that is to say are owing to an internal and vital cause brought into action by an external agent. Attraction then does not affect the ponderable matter of a vegetable, but acts on its nervimotility.

Light exercises a special agency over the direction of vegetable growth—as we know from many familiar examples; but this influence is not a paramount one. When a plant is pulled up, which lives under the operation, and is laid on the earth horizontally in a dark place, as in the instance of the onion or leek, the stem undergoes a curve, and the superior part takes its former direction towards the heavens. The same thing occurs whether these vegetables are placed in water in the dark or in daylight. The regaining of their upward direction by the stems is attributed to the conjoined operation of gravitation and light.

The tendency of the stems and branches towards the light is, according to Dr DUTROCHET, in virtue of their green colour; and if roots do not in general evince the same tendency, it is owing to their want of coloration. Here, however, we are lost in first causes: light, which is the principal though not the only agent of the coloration of the stem and its appurtenances, has no such influence over roots, whatever be their exposure. Thus an aquatic plant will always have white roots, although they are exposed to the light as well as the submerged leaves which are of a green colour. Singular as it may seem, the green part of some roots, as of the *mirabilis jalappa*, will be constantly directed towards the light, and the white stems, as of some aquatic plants, will grow downwards towards the earth. Hence it is not in virtue of their properties as stems or trunks that plants have an upward direction, but on account of their coloured parenchyma: nor is it by their office of roots that these parts descend into the earth, but because they have a colourless parenchyma.

This serves to explain another phenomenon which has greatly engaged the attention of botanists. We mean the constant direction of the upper face of leaves towards the heavens, and of the lower towards the earth. The tendency depends entirely on the difference in colour: the upper, being generally of a deeper green, seeks the light; the lower, of a fainter hue, inclines to the ground. If the conditions be reversed, as happens in some of the gramina, the lower surface is turned upwards by a kind of torsion of the body of the leaf. When the two surfaces of a leaf are of an equally deep tint, neither has an upward direction: it is the point of the leaf which displays the tendency. The petals of flowers are subjected to the same law of direction which governs the leaves of the plant.

These movements, like the others already indicated, are not, however, effected by the attraction of light or any such physical agency, but are the result of an internal spontaneous movement excited by a particular stimulus operating on the nervimotility of vegetables. This is proved by the fact of its becoming indifferent, to the sensitive plant for example, which side of the leaf is uppermost, if it has been kept in a dark place so long as to lose its motility, and impressibility by stimuli.

The tendency in the leaves of plants to remove from any cover or shelter under which they may be placed is explicable on the same principle, viz. their proneness to direct one or other surface to the light. If the leaves be kept in profound darkness, they make no effort to withdraw themselves from their partial covering or shelter.

The *sleep of plants* is explained by Dr Dutrochet on the principles of vitality. He supposes that the different direction affected by the leaves and flowers of many plants during the night, to that which they preserve in the daytime, is the result of a great and rapid diminution of their nervimotility. Hence any violent nervimotor agent, which consumes rapidly this inherent property, such as a shock or burning, produces a *diurnal sleep* differing in no one particular from their *nocturnal sleep*. He supposes that light is both a source of the renewal of vegetable nervimotility, and an agent for its exhaustion, or a nervimotor; an opinion which, however correct, he does not very clearly substantiate by experiment. He tells us that in the second section of this work, he has shown how light repairs or renews nervimotility in the sensitive plant, which darkness had destroyed or exhausted. We have given his language on that occasion, in which he says that although motility was diminished and destroyed, nervimotion was no otherwise affected than in the extent of its transmission (p. 83). There is in this some little contradiction, which as far as we have seen is not cleared up in the text.

In the fourth section, on the influence of rotatory movements over the peculiar direction of the parts of a plant, the author gives the results of several experiments, of a nature similar to those already performed by that celebrated vegetable physiologist Mr Knight. He planted seeds in a portion of earth which was kept by a suitable apparatus in continued rotation, and he found after several trials, 1, that the radicle of the germinated seed obeyed the impulse to which it was subjected, and constantly tended in the centrifugal direction, the result of the circular movement, by growing towards the circumference; and 2, that the plumule *reacted* against this tendency, and grew in a direction diametrically opposed to the centrifugal force,—that is, towards the centre. Leaves exposed to a similar rotatory movement constantly directed their superior surfaces towards the centre, and their inferior ones towards the circumference. Hence it would seem that the two opposite faces of leaves have quite contrary conditions of vitality—the superior one exhibiting the same as those of the plumule in being like it directed to the centre, and the inferior imitating the radicle, in being directed to the circumference.

"We now see," says the author, "that under the influence of the same nervimotor agent, the plumule and radicle take diametrically opposite directions. These two parts thus differing in their position have then a different nervimotility, if not in its nature at least in some of its conditions; since the one tends to produce obedience, and the other reaction, in reference to a nervimotor agent which undergoes no variation. There is a nervimotility a principle of obedience, and a nervimotility a principle of reaction. These two opposite modes of nervimotility being located in entirely opposite parts, we cannot fail to recognise therein a phenomenon quite analogous to what is called polarization in natural philosophy. Nervimotility or rather its unknown agent presents truly two poles in vegetables; the roots being the seat of the *obeying*; the stalks of the *reacting pole*. These said poles are located on the two opposite sides of the leaves; the *reacting pole* is on that side usually directed towards the heavens and light; the *obeying* pole is on the surface which looks towards the earth. We have seen, in the preceding section, that the direction of these parts, which are considered as different poles, invariably coincides with a difference *plus* and *minus* in their coloration. The obeying pole is always of a less deep colour than the reacting one; and consequently there is an excess of certain conditions in one, while they are in less degree in the other. I am inclined to believe that these visible differences, *plus* and *minus*, coincide with corresponding differences, that is to say *plus* and *minus*, in the conditions of nervimotility."

Dr Dutrochet next proceeds to a consideration of the intimate structure of the nervous and muscular systems of animals, and the mechanism of vital contraction.

The nervous system of animals, observed in its microscopical elements, is composed essentially of agglomerated corpuscular globules, which seem to be cells of extreme smallness containing a nervous or medullary matter, concrescible by heat and acids. The two small symmetrical bodies at the summit of the nervous system of the gasteropodes mollusca, corresponding to the brain in the more perfect animals, exhibit, after their fibrous envelope is removed, two small pulpy kernels. These, especially of the *helix pomatia* and the *limax rufus*, are found, on examination under the simple microscope, which the author prefers to the compound one, to consist of agglomerated globular cells, on the sides of which are seen a great quantity of globular or ovoid corpuscles (fig. 20. of the work). These last are evidently small cells, filled with a semi-transparent medullary or nervous matter of a distinct white colour. The globular cells to which these are attached likewise contain a nervous medullary substance which, as far as we can judge by the microscope, is of a grayish colour and semi-transparent. Here we have an analogous distinction to that met with in the gray and white matters of the brain of vertebral animals.

The nerves of the *helix pomatia* and *grisea* present externally a cellular tunic tolerably thick and semitransparent: the agglomerated cells of which it is composed are globular and contain a diaphanous and colourless fluid, and the sides of these cells contain corpuscles also diaphanous (fig. 21). In the centre of the canal formed by this cellular envelope is the nerve properly so called (the tissue of which is represented in *b*, fig. 21). It is composed of an immense quantity of nervous corpuscles of very minute size, adherent to two sets of fibres, the one longitudinal and larger, the other of extreme tenuity irregularly distributed in the spaces between the preceding. It is the nerve proper which alone enters into the interior of the organs; the cellular envelope is contiguous with an analogous one investing all the organs.

The nerves of the frog seem to be composed of transparent filaments, surrounded by nervous corpuscles, an arrangement very nearly identical with that just designated in the nerves of the *helix pomatia;* and not dissimilar to what we see in vegetables, in which globular corpuscles cover tubular cylinders.

The brain of the frog is entirely composed of an agglomeration of nervous corpuscles similar to those of the nerves. The only difference is in the proportion of the organic elements.

Here the author allows himself the following inductions. Since the brain, an organ peculiarly destined for the production of nervous power, is composed of nervous corpuscles; and the nerves, specially intended for the transmission of the nervous power or nervimotion, are entirely composed of nervous fibres, we are entitled to regard the nervous corpuscles as the generating organs of the nervous power, and the nervous fibres as fitted for its transmission. And since in vegetables we have seen that nervimotion is transmitted by the medium of the sap, we may infer that the *nervous* fibres of animals are tubes filled with a peculiar fluid by which nervimotion is accomplished.

Polypi have no nerves properly so called, yet they manifest by their movements the influence of external agents over them. This apparent contradiction is explained away by the author, who avers that in their transparent and apparently homogeneous pulp, we can distinguish, by the aid of the microscope, a large quantity of granulations resembling completely the nervous corpuscles of other animals and still more of vegetables.

The attention of the author is next directed to the structure of muscles and their motility. The term *muscular fibre* ought, he thinks, to be restricted to the filiform organs of which the muscles are directly composed; while by muscular fibrils he would understand the filiform organ smaller than what is observed in the intimate tissue of the muscular fibres. The rectilinear assemblage of globular corpuscles in the intimate structure of muscular organs is called by him *articulated muscular corpuscles*. All this is very minute, but not we think very readily understood. The muscular fibre is, he says, seen in its greatest simplicity of organization in some of the inferior animals, such as the craw-fish. On separating by means of a needle, the muscular fibres of the tail of this animal, and then subjecting them to the microscope, he found that the tissue consists of transparent fibrils arranged longitudinally, and in the intervals of which are numerous transparent globules, designated by him as *muscular corpuscles*. These are filled with a diaphanous fluid and interposed between the fibrils and applied to their surface, though their adherence is not very close, since some fibrils are entirely destitute of them.

The heart of inferior animals has no muscular fibres properly so called, but only fibrils and muscular corpuscles. In that of the lobster, the arrangement by *articulated muscular corpuscles* prevails. The heart of the frog, different from the other muscles of this animal, is also destitute of proper muscular fibres.

After having studied the intimate structure of the muscular tissue, the next point is to determine the mechanism of its motion. For this purpose Dr DUTROCHET selected the muscular fibres of certain insects, such as the horn beetle (*lucanus cervus*), which are completely isolated from each other. He discovered, however, that the contractile movement of the fibre depends on an internal mechanism, which it is impossible to see on account of their want of transparency. He determined, therefore, to direct his inquiries to the muscular organs which have no *fibres*, but the tissue of which regarded in its entire state is composed like the muscular fibres properly so called. This is the arrangement in the heart of the batrachia and gasteropodes mollusca. As the results of some of his experiments we may state the following. The contraction of a muscle by acids is to be regarded as a true vital process, and not the effect of mere corrugation. The muscular fibres of the thigh of a frog being placed in water and examined by a microscope were seen to be curved and twisted on one another like so many small worms. This last kind of action is the result of a peculiar vital property of the fibre, since for its production there was neither *sensation* to determine the movement nor *volition* to execute it. The tendency of the fibre to change in the relative position of its integral parts is by a movement on one side by which it becomes concave, and there results *incurvation*. If acids provoke the contraction or incurvation of the fibre, alkalies on the other hand bring it back to its first position. This alternation of incurvation and straightening is seen to take place under the influence of vitality alone. Contraction in the corpuscular muscular tissue consists in a *sinuous incurvation;* by which we presume the author means an alternation of concave and convex. In other cases there seemed to be merely a closer approximation or condensing of the muscular corpuscles without any turn or curve. The shortening then of the muscular tissue which constitutes contraction depends on a double cause, viz. corpuscular condensation, and the folding or sinuous incurvation of the tissue. Contraction and coagulation are merely different degrees of one and the same phenomenon. Acids both produce coagulation and give rise to contraction; alkalies produce relaxation of muscles and restore fluidity after coagulation.

Animal irritability and *vegetable irritability* are two phenomena essentially identical; both depending on the *incurvability* of their organic tissue, or of the vital faculty which it possesses of being curved and maintained in this state of curvature by its elastic character. In vegetables the incurvation is for the most part *simple*, that is of a single curve; whilst generally in animals it is sinuous. We are not to restrict incurvation to those vegetables called *irritable*, such as the sensitive plant, but to extend it to all which by their movements assume the positions of *waking* and *sleeping*, and which by tendrils attach themselves to the nearest support.

The knowledge acquired of the cause of this incurvation in animals enabled Dr DUTROCHET to discover the origin of the phenomenon in vegetables. We have just seen that the muscular tissue is composed of agglomerated vesicular corpuscles, sometimes in linear order and at other times distributed without method, and which are, after being rendered slightly opake by acids, dissolved in them, a distinction this between them and the nervous corpuscles which are insoluble in such menstrua. Now it will be remembered that on examination of the articulation or *bourrelet* of the sensitive plant, there was seen a great number of globular cells in a line, which were filled with a fluid concrescible by cold nitric acid but soluble in hot. These globules are true *muscular corpuscles* larger than those of animals, but resembling them in their linear arrangement and chemical composition.

We shall postpone for the present the analysis of the remainder of the last and concluding section on muscular contraction and the coagulation of the blood, in order to connect the author's views with the observations and experiments on the same subject by Messrs PREVOST and DUMAS. The pages yet allowed to us for this article will be taken up by a succinct notice of the second work of Dr DUTROCHET *on the immediate agent of vital movement.*

His first topics of inquiry are into the direction of the sap in plants and the cause of its progression. It is customary to speak of the ligneous or woody fibres as the part by which the ascending sap is carried; but the author, after pointing out the vagueness of this expression, shows us that it is by the lymphatic or corpusculiferous tubes of the central ligneous portion that the process is performed: the closters have no share in it. The lymphatic vessels are never obstructed or closed up in the hardest and oldest layers of the wood, or what the author calls *duramen*, to distinguish it from the alburnum, or sap wood. The lymphatic vessels of this latter transmit the sap with equal facility as those of the duramen. The cortical system of the plant destitute as it is of corpusculiferous or lymphatic vessels does not participate in this function.

The tracheæ, as was observed in the first work noticed in this article, are the vessels in direct functional connexion with the leaves, in which they are found in large numbers. Knowing then that the leaves are in a measure the lungs of the plant, we are allowed to believe that the fluid vivified in them is carried by the tracheæ into the interior of the stalk or trunk ; for these vessels are seen to extend from the leaves to the medullary sheath of the young branch which bears them.

The leaves, flowers, and fruits, are the parts to which the ascending sap is definitively conveyed. Here the greater part of this fluid escapes by transpiration into the atmosphere, in which it is dissolved. A portion submitted to organic elaboration is converted into a nutritive juice. The leaves are more especially charged with this last important function, and hence if they be destroyed there is a cessation of nutritive action in the vegetable, as well in the growth of the stalk or stem as in the development of the fruit. The elaborated sap being formed in the leaves must then take a descending direction—a fact proved by a very familiar experiment. If we remove a ring of the bark from a branch of a tree, the part directly below the denuded portion ceases to increase in diameter, whilst that above the ring receives an augmented growth owing to the forced stasis of the nutritive fluid, the tendency of which to descend is manifested by the projecting circle at the upper part of the annular decortication. The increased nutrition of the fruit in the branch above is additional confirmation of this fact. It was at one time believed, as an inference from this experiment, that the bark was the only part through which the descending sap was conveyed. Mr Knight, however, in a paper published in the Philosophical Transactions in 1806, suggests the probability of the occasional or accidental descent of the sap in the alburnum, when such a distribution is necessary for the preservation of the plant. Dr Dutrochet gives us a more physiological and consistent explanation of the phenomenon, by showing that although the lymphatic vessels, those for the ascending sap, prevail in the greatest proportion in the alburnum, yet we also meet with closters in its substance, which certainly contain elaborated sap. It is not then necessary for us to admit with Mr **Knight** the *inverted* action of the vessels of the alburnum; for Dr **Dutrochet** has shown the ligneous tissue of the central system of the plant can of itself carry on the downward transmission of the sap. This, however, is most readily and naturally accomplished when the wood is clothed with bark.

After all we must not regard the elaborated sap as always taking a *descending* course. It becomes *ascending* in the peduncles of the fruit, and for the elongation of the stem or trunk and development of the buds. To Mr **Knight** we are indebted for a knowledge of the fact, that the ascending sap, or the lymphatic fluid, dissolves a certain quantity of the elaborated juice which had been preserved in the wood during the winter. This last was contained according to Dr D. in the closters.

In reference to the functions of the medulla or pith of the plant, the author thinks that, composed as it is so much of globular corpuscles, which he regards as nervous bodies, it performs the part of giving additional life and vitality to the buds and young branches. He admits also, to a certain extent, the opinion of M. **Dupetit—Thouars**, that the medulla of the older or mother branches contains in its cells nutritive matter in reserve for the other and younger parts of the plant, especially the buds, which he calls *fixed embryos.*

It follows from the facts stated in the work, that the lymphatic sap and elaborated sap have each of them special organs of transmission, and by these they tend to a general diffusion through the plant. If the lymphatic be for the most part ascending, this is owing to its being chiefly derived from the soil; and if the elaborated be chiefly descending, we find the cause in its originating exclusively from the aerial part of the plant: but at times each of these two kinds of sap may take an inverted course. *There is not, then properly speaking, a circulation in vegetables*; that is to say there is no return of the mass of the fluid to the point whence it set out; though in consequence of their different origins, radical and foliated, the two kinds of sap, ascending or lymphatic, and descending or elaborated, usually flow in opposite directions.

The effusion of the sap between the wood and bark in the dicotyledon plants, and the consequent formation of new layers of *alburnum* and *liber*, are accomplished, in the author's opinion, by the medullary rays of the central or ligneous, and of the cortical or external system, which, meeting, pour out the two kinds of sap.

Connected with our inquiry into the course of the sap, is a knowledge of the formation of the tubes which serve as canals for this fluid. The origin of the lymphatic vessels, including the *porous tubes*, or *divided tracheæ*, is evidently cellular, the cavities of the cells having no direct communication with each other. The continuous lymphatic canal, subsequently formed, only becomes so by the disappearance of the transverse partitions constituting the cells. This explains the difference in the effects of incisions in the old wood of a vine, and in the young branches in the spring. The discharge from the first is copious and strong, that from the latter small and feeble, on account of the *vis a tergo*, by which the sap is driven out of the wood, being obstructed by the cellular arrangement in the new portion.

Independently of this essential organic element of a cell, vegetables have no other but the tracheal, different in its origin and nature.

In the second chapter, headed ' *Trepidation*,' the author investigates the phenomenon which was supposed by a Prussian botanist, M. SCHULTZ, to constitute a true circulation of yellow juice in the great celandine (*chelidonium majus*, *L.*). Dr DUTROCHET at first supposed the whole to be an optical illusion, but a repetition of his experiments satisfied him of the reality of a molecular movement in the ribs of the leaves of the great celandine, which he qualifies by the term trepidation. He cannot, however, admit its being a true circulation; since, among other objections, he found it to continue with unabated rapidity for several days after the separation of the leaf from the plant, and even until the former was almost withered. Still trepidation is a vital movement, destroyed or suspended by a reduced temperature, as when the thermometer is near the freezing point. The author compares it to the molecular movement of the blood in animals immediately after their death, a phenomenon which he observed in the blood vessels of a mouse's ear, and in the mesentery of the same animal. That it is not a true circulation is evinced by the fact of there being no current; since, although the two extremities were cut there was no issue of fluid from either. We have little doubt but that these deceptive appearances misled WILSON PHILIP, and others, into a belief of what he calls capillary circulation in the mesentery of certain animals, after the removal of this part from the rest of the body.

The third chapter is occupied with investigating the causes of the movement of the sap. Of the hypotheses which have been at different times accredited, the author thinks it worth while to notice two only. The first supposes the rise of the sap to be the effect of the capillarity of the vegetable tissue; the second explains the phenomenon by the alleged contractile power of the vessels which thus propel the fluid onwards.

That the sap is driven forward by a strong propulsive movement was long ago shown by HALES, and has more recently been demonstrated in a conclusive manner by MIRBEL and CHEVREUL. The former, on adapting a tube filled with mercury to the branch of a vine, the anterior extremity of which had just been cut off, saw the sap take the place of the metal and raise it to the height of 33 and 38 inches above its primitive level. The two latter gentlemen in a similar experiment found the mercury to rise 29 inches; that is to say, as high as it is kept by the whole pressure of the atmosphere. If a vine branch, thus discharging sap so freely from its extremity, be cut off lower down and immersed in water, this fluid will be drawn up by the open vessels of the lower end, but no sap will escape from the upper one. Hence there would seem to be two causes operative in its rise; the first is an impulsion *à tergo;* the second is dependent on the action of the internal organs of the plant on the liquid which it absorbs.

It is with regret that we find ourselves compelled to pass over many interesting experiments and reasonings of the author on this subject, and must content ourselves with a notice of some of his leading positions. When the cut end of a vegetable or branch is immersed in water, this fluid is absorbed and distributed to all its parts, maintaining life and freshness in them. Observation teaches that the absorption, in this case by the stem, is proportioned to the evaporation from the leaves, the removal of which would be attended by the cessation of both functions. Light exerts a powerful influence over the respective quantities of absorbed and evaporated fluid; nor is it necessary that heat should be conjoined to produce this effect. Absorption is rather greater during the night,—evaporation during the day. The partial vacuity then of the vessels of the upper part of the plant by evaporation, is one of the determining causes of the absorption effected by those of the lower portion. Another inference is, that the capillarity of the vegetable cannot be the sole cause of the absorption of the water and its rise in the stem of the plant, since this property does not vary, and we can see no reason why it should introduce more water during the night than is dissipated by evaporation, and why the contrary should be the case in the daytime. There is then evidently a vital action modified by the presence and absence of light, and destroyed by a too great desiccation of the plant and evaporation, not merely of the water it contains, but of its *organic fluids*. Moderate desiccation, as when a plant is slightly shrunk and drooping, enables it to absorb with increased avidity, and become even more turgid than before. One notable distinction therefore between a *dead* and *living* plant is, that, notwithstanding the vegetable tissue of the former may be as full of water as its capillarity requires, it is still far from being *turgid*, it is *flaccid;* whereas in the latter the water fills its capillary cavities to superabundance, and produces a state of general turgescence. We are, therefore, allowed to believe that the turgid state of the small hollow organs of a plant is the necessary condition for the rise of the sap by *affluxion* (*adfluxion*). By this term, the author means the flow of the sap towards a particular place where a cause operates to invite or draw it. Thus, if a branch be cut off and its lower extremity be immersed in water, it is only by *affluxion* that the water rises up through the stem to the leaves. But in a plant which is connected with the earth by means of roots, there is, independently of the movement of the sap by *affluxion*, a progression by *impulsion* the evident effect of a *vis a tergo.*

Inquiries into the nature of this second motive power led the author to the following discoveries. The moment in which the stalk of a vegetable was cut off close to the earth, was that in which the sap ceased to flow from the upper part which had been exposed by a transverse section of one of its branches. There can therefore be no cause of impulsion in the stalk or trunk, as the case may be. It was found after continued sections of the roots, that the sap still continued to flow from them, unless their capillary portion or the spongy terminations of the radicles were interested. This spongy body (spongiole) is really the organ of impulsion for the rise of the sap in vegetables, and it is, moreover, as proved by BONNET, the special organ for the absorption of water.

Before attempting to determine in what the propulsive action of the spongy bodies of the radicle consists, the author takes a view of the structure of these parts. The spongy bodies of the vine observed in the spring are of a whitish colour, and of a delicate and tender tissue, which is entirely of a corpusculiferous cellular nature. The central portión of it is articulated in a longitudinal direction and is continuous with the central system of the radicle, and its articulated cells are the elements of tubes likewise articulated, called, as the reader has already seen, *corpusculiferous* or *lymphatic vessels*, and which are destined exclusively for conducting the sap upwards. The cellular tissue external to this and forming a continuation of the cortical system of the radicle is irregular, very transparent, and covered with corpuscles of exceeding minuteness. There is formed every spring a new spongy body, whitish and delicate, issuing from the one of the year before, which last becomes a constituent part of the radicle. On the following year the cortical portion of this new formed radicle dies, so that the remaining central or ligneous part is smaller than the very termination or spongy body, the delicate cellular layer of which is more especially the organ of absorption.

The author next investigates the foundations of the two hypotheses which have been made to explain the state of pressure to which the sap is subjected in the lymphatic vessels of a plant. The first is that the air often mixed with the sap does by its expansion, and in proportion as it is formed, communicate an impulsion to the fluid. The second rests on the supposition of the hollow organs, which contain the sap, having a contractile power like that which the heart exercises on the blood of animals. The fact of air being mixed with the sap is admitted, but is, on the author's showing, incompetent to produce the effect in question. The contractility of the lymphatic vessels, plausible enough on first annunciation, is disproved by the following among other considerations. The fig tree abounding in a large quantity of milky juice might be supposed, so long as its branches are herbaceous, to give it out from a cut extremity by the contraction of its vessels; but when we find the juice poured out abundantly from the tissue hardened into wood, and even from the ligneous portion of the central system so resisting as to break with fracture and noise, we cannot for a moment suppose the expulsion of fluid to be performed by such a mechanism as vascular contraction.

We now approach a most important part of Dr DUTROCHET's work. It is the chapter entitled "*Observations and Experiments on the Cause and Effects of the Turgid State.*" The author found that there projected from the surface of a wound made by cutting off the tail of a small fish, which he preserved alive in a vessel of water, a production similar to a kind of aquatic moss with tolerably long filaments, each of which was terminated by an enlargement very perceptible to the naked eye. Some of these transparent filaments, the enlargements of which resemble the capsules of vegetables, and terminate in a point, and are completely opake, were put in a watch crystal for more minute inspection. Dr D. soon saw some of these capsules expel through an opening at their point a great many globules. During this process the cavity of the capsule was emptied at its lower part alone, opposite to the point whence issued the globules, and the mass of which still filled the upper part of the capsular cavity and seemed pressed and strongly driven upwards by the accumulation of water in the inferior portion; hence the expulsion of the globules was evidently not due to any contraction of the capsules. The water seemed, says the author, by its introduction into the inferior part of the capsular cavity to perform the office of the piston of a syringe, for the driving up and expulsion, through the point of the capsule, of the collection of globules which had at first completely filled it. The globules were believed by him to be animalculæ, similar to those called by Bory de Saint Vincent, *zoocarpees*. He found, however, that on immersing pieces of any animal matter into the water in which the fish had been kept, that they were soon covered with a similar production as in the case of the wounded surface, and hence that this is not dependent on the vitality of the animal. Some days are required for the plant, since such it is, to acquire the maturity necessary for it to give rise to the emission of the globules, or as Dr D. believes them to be, seeds contained in the capsules. Regarding the water introduced into the inferior part of the capsular cavity as the evident mechanical instrument of the *vis a tergo*, by which the expulsion of the collection of globules is effected from the remainder of the cavity, he asked himself, whence comes this water, and what is the power by which it is driven into the interior of the capsule? But for his being able to observe with the naked eye, a similar phenomenon in the animal kingdom, he would probably have been content to have noted what we have just described, as an isolated and inexplicable fact. A small spermatic sac deposited in copulation by the male, is occasionally, if the animal be alarmed at the time, expelled from the female organ of the snail. These sacs are like small retorts, much curved and larger at their closed than open extremity. When placed in water they are gradually emptied of their sperm, which escapes with effort from the neck, while the water occupies the lower and largest portion.

Here, as in the former case, the accumulation of water in the lower part of the sac, through the sides of which it was introduced, acted evidently as a mechanical agent for the pressure or expulsion of the spermatic paste through the upper part or neck. All doubt respecting the nature of this expulsive force was removed by the author witnessing the issue of the last portion of the sperm from the sac with a current of water, very sensible by the repulsion which it exerted on the light bodies floating in it. The current became afterwards more feeble, and entirely ceased after the expiration of a minute. The sac or little retort thus emptied of its contents did not display any contraction of its sides nor any shrinking. To give still greater assurance of the correctness of the experiments, another sac was placed in water with its neck uppermost, so that the emission of the sperm could not be attributed to a simple flowing out after dilution in the surrounding liquid. Things passed in the same way as at first, except that rather more time was required for the entire completion of the process. It would seem then that water accumulated gradually in the interior of small organic cavities, becomes there a mechanical agent of impulsion, a true *vis a tergo* for the expulsion out of these small cavities of the substances which they formerly contained. To this *physico-organic* or *vital* action, the double effect of which is to render turgid the small hollow organs, and to produce an expulsion of the substances which they before contained, Dr Dutrochet has given the name of *endosmosis** (*endosmose*).

Once in the path of inquiry, the author went on multiplying his observations and corroborating the inferences from the former phenomena. The cœcum of a chicken, after being well washed and cleansed internally by frequent injections of pure water, was half filled with milk, and its open extremity tied with a ligature. It was then immersed in a vessel of rain water, its weight together with the contained milk being 196 grains. Twenty-four hours afterwards, it was withdrawn from the water, when it weighed 269 grains, having gained by immersion 73 grains of water introduced into its cavity. It was again placed in water which was renewed night and morning. After twelve hours the weight was increased to 313 grains and the cæcum was very turgid. From this time it began to lose weight and turgidness, and its emptying going on caused a shrinking of its sides. Thirty-six hours after the commencement of this diminution of weight the intestine only weighed 259 grains. The experiment was then terminated, the cœcum was opened, and the milk found curdled and putrid. The temperature during this time ranged from 74 to 79 degrees Fahrenheit. Having emptied and well cleansed the interior of the cœcum by injections of fresh water he half filled it anew with fresh milk, and placed it as before in water. After the lapse of twenty-one hours, he discovered an augmentation of weight equal to 21 grains. From this time, however, there was a rapid diminution. At the expiration of twenty-four hours more, the intestine was opened, and the milk was found to be decomposed and putrid.

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* From ἔνδον *inwardly*, and ὠσμος *impulsion*.

Though as a general rule, there ought to be an organic fluid denser than water and possessed of peculiar chemical qualities, constituting a *healthy state* in a hollow organ, in order to give rise to endosmosis, yet even this is not absolutely essential. Rain water, introduced into the cavity of a cœcum in place of milk, will give rise to endosmosis, which is however suspended in all cases in which the contained fluid becomes putrid, or the organic membrane itself is similarly affected. Continuing his trials, the author found that endosmosis would go on, even though the hollow organ were completely empty; hence he infers, that it is an action performed by the tissue of the sides of the hollow organ itself. Nor does this depend on the particular superposition of the intestinal membranes. When the cœcum was turned inside out, the sac thus formed allowed of the introduction of water. But if an organic liquid, such as albumen, still denser than milk, be introduced into the cœcum, the quantity of water which passes through its sides will be even greater than in any of the former experiments.

A new phenomenon was next noticed by the author: when the endosmosis ceases in consequence of the putrefaction of the contained fluid, the water, in place of entering into the cavity as before, or even remaining there after its entrance, issued out and that with some rapidity. His suspicion, soon confirmed by experiment, led him to the admission of a new action opposed to the former one. Thus when the denser of the two fluids is in the cavity, the water is introduced by *endosmosis;* but if the fluid without be the denser, then the contained one is carried outwards by an inverse action designated by the term *exosmosis** (*exosmose*). These two actions were uniformly connected with the chemical nature of the fluids employed, alkalies in the cavity always producing endosmosis, and acids exosmosis. The belief at this time entertained by the author, that the movements of the fluids depend on electrical currents, was fully confirmed by experiments. On taking a chicken's cœcum, and introducing the negative pole into its interior and placing the positive outside, so that the side of the intestine was the communication between the two poles, and then putting the galvanic pile in action, the cœcum being immersed in water, the cavity before empty now became turgid by the passage of water through its sides in the direction of the electric current, that is, from the zinc or least dense to the copper or most dense pole. The result was reversed by putting the positive pole within the cavity, and the negative outside, as in the case of the less dense fluid being in and the denser out.

Dr D. compares the hollow organs to Leyden jars with permeable sides; and as the current of fluid is always towards the negative pole, there will be endosmosis when their interior is in a state of negative electricity, and exosmosis when it is positively, and the exterior negatively electrified. Organic tissues are proved to be better adapted than inorganic porous bodies for the formation of an electrical current, which gives impulse to the water. Farther experiments satisfied the author that the two processes of endosmosis and exosmosis were constantly going on with variously proportionate energy at the same time. The intensity of endosmosis is increased by an elevated temperature.

Applying now with the author the above described phenomena to vegetable and animal statics, that is to the course of the sap in plants, and of the lymph, chyle, and blood in animals, we shall find ourselves with a more extended horizon for observation than before. The distention of the sap vessels of a vegetable is due, our readers will remember, to a *turgid state*, dependent on a true vital action. Now the cause of this turgid state is no other than endosmosis, in virtue of which the water in contact with the spongy bodies of the radicles enters into their vesicles or cells, which contain a denser organic fluid. The water thus imbibed propels that precedingly introduced, and causes necessarily an upward movement of the sap in the vessels, which it was shown were continuous with the articulated cellular tissue of the root. This continued progression by *impulsion* explains the pressure to which the ascending sap is exposed in the tubes of the vine. Another movement, also the result of endosmosis, is that of affluxion or pumping, which resides in the leaves. It has been already shown, that evaporation from the leaves invites the afflux of fresh fluids from beneath; but, for this process to be performed they must possess a certain degree of turgescence, that is, a certain amount of organic fluids, which implies the action of endosmosis. The leaves and spongy bodies exhibit then, in an inverse manner, the union of the two effects of endosmosis. From their combined operation result on one side the ascending, and on the other the descending course of the sap. The vessels themselves are for the most part inert in these operations; since, in addition to the facts already assigned, it has been found, that a liquid has ascended in the stalk of a plant which was entirely dead, the leaves alone preserving their vitality and causing the movement of affluxion.

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* From ἐξ *out of*, and ωσμος *impulsion*.

Anticipating the remarks of the author on absorption in animals, we may regard it as performed by an action analogous to, and even identical with that by which the progression of the sap is accomplished. Both absorbents and sap vessels receive their fluid contents by filtration through spongy bodies; the membranes for the former and the capillary terminations of the radicles for the latter. The fluid in both cases passes from vesicle to vesicle by endosmosis, and while maintaining them in a state of turgescence in excess, it acts as an impelling power to the fluid in the tubes connected with the vesicles. It might at first be thought that the knots in certain classes of vegetables as the gramina, and the lymphatic and lacteal glands in animals, would impede the course of the fluids in their respective systems of vessels. A little reflection satisfies us that the reverse holds good. The knots have agglomerated vesicles, each of which exerts its particular endosmosis, rendering them ends of affluxion and origins of impulsion. The glands are furnished with a capillary tissue, the endosmosis of which produces the afflux of fluid contained in the *vasa afferentia*, and at the same time the expulsion of the fluids precedingly introduced, and hence an impulse *à tergo* for the *vasa efferentia*. These bodies are then, in fact, organs of afflux for the fluid below and of impulse for that above; they constitute motor organs for the fluid contained in the vessels.

Observation shows that there are *elective absorptions*. Thus in the alimentary canal, the chyle alone is absorbed, though it be mixed with excrementitious or fecal matter; because the former produces endosmosis, whereas the latter has a tendency to cause, and does in fact cause exosmosis, or a secretion in place of absorption.

This greater density of the substance applied than the fluids of the animal economy, or its possessing certain chemical qualities, which we have seen may be equivalent to excess of density, will explain perhaps why very active medicines given in large doses often fail to produce any effect: they are not absorbed, or very slightly so, precisely on account of the largeness of their dose.

Exhalation, either partial or general, is of course the result of exosmosis, and according as this prevails over endosmosis, so will exhalation be superior to absorption, and *vice versa.*

Nutrition is regarded by the author as the result of endosmosis. Each vesicle entering into the composition of an organ has in its interior a fluid denser than the sap of a plant or the blood of an animal, or which is of such a chemical composition, that the nutritive circulating fluid is attracted from the vessels, and a part of it is introduced into the vesicle, increasing thereby its size and giving it growth. But, as already explained, the inward is not the only current, since electricity, the cause of the phenomenon, gives rise to two currents in opposite directions, and of unequal force; and as that of introduction or endosmosis is stronger, the vesicle or cell is always filled in excess and preserves its turgid state. The feeble exosmosis is in degree aided by the violent entrance of new fluids, which must in consequence expel some of the matters before introduced. What is said of one vesicle or cell applies to all, the union of which constitutes the organ.

Secretion may be regarded as nearly an analogous process. Each vesicle secretes its contained liquids; that is, it only allows molecules of a particular nature to permeate its sides.

The sanguiferous system in animals or its minuter ramifications, keep up a kind of *irrigation* of the organs, and carry to the alimentary vesicles the new organic materials, which are received by a kind of infiltration, and not, as generally supposed, by vascular orifices. But whilst the vesicles thus receive by endosmosis new molecules from the blood, they expel by exosmosis others which are carried into the general current.

The renewal of parts by absorption, and exhalation and secretion, constituting nutrition, takes place in the contained substances of the vesicles—the changes of composition and decomposition are in the *contained* not *containing* parts. If the latter were renewed like the former, it is probable that death from old age would never take place, for the being would never become old.

The author has some very interesting observations on the circulation of the blood in animals, and on inflammation, many of the phenomena of which are explicable on the principles before laid down. The sanguiferous system ought, he contends, to be regarded in its entireness as a cavity without issue, admitting of neither the entrance nor exit of any matters except by infiltration. Arterial extremities and venous radicals are physiological fables. The complete continuity of arteries and veins is visible through the microscope, in young salamanders. As regard the motive powers of the circulation, we must admit in the case just cited that the impulse of the heart is sufficient to communicate motion to both the arterial and venous blood. It was from having studied the circulation chiefly in reptiles, that SPALLANZANI was led to the belief of the paramount agency of the heart to the progression of the venous blood. But when the arteries are subdivided into innumerable capillary ramifications before their termination in veins, the impulse of the heart becomes gradually insensible, and the blood is moved under the influence of a new power. The common opinion of there being a contraction in the capillaries, and of this being one of the motive powers of the circulation is purely hypothetical. It would presume a *diastole* and *systole* similar to that of the heart. Dr D. has paid great attention, by means of the microscope, to the capillary circulation, but he could never perceive the least movement of contraction or dilatation in the capillary coats. He admits their elastic power, by which they react on the blood which distends them, but this is very different from true contraction. He thinks that the explanation of the course of the venous blood being caused by atmospheric pressure and respiratory movements, as taught by Dr BARRY, can only be an accessary one peculiar to mammifera and birds, which alone form a vacuum in breathing. It is by the action of endosmosis that the blood receives a fresh impulse in the capillaries. The blood of the arteries is *drawn* into their capillary ramifications, and hence the reason of the emptiness of the arteries after death when the heart is no longer active. The blood of the veins receives in the capillary ramifications an impulse, the cause of which has, until the present time, been vainly sought for by physiologists. This cause, says Dr D. is evidently to be found in the affluxion of the fluids which drive forward, by replacing them, the fluids before existing in the capillary cavities.

As there are no vesicles or cells in the capillaries between the arteries and veins, we could have wished that the author had specified the manner in which endosmosis takes place in the first mentioned system of vessels, between the blood in which and that in the arteries he has not pointed out any difference in density or chemical properties, conditions these for the support of the turgid state and the movement of affluxion. He seems to have forgotten that from an artery to a vein there is a continuous tube, which can not, as far as he has yet shown us, be the seat of either endosmosis or exosmosis.

Inflammation he regards as the exaggeration or morbid increase of the physico-organic action so often mentioned: it is he thinks a *hyperendosmosis*, a name which he also applies, though we fear not very correctly, to the turgescence of the erectile tissues. Erectile hyperendosmosis has indeed, he tells us, but one occasional cause, viz. nervous influence, whilst morbid hyperendosmosis depends on numerous occasional causes. One of the most common of the latter is the introduction into the organic tissues of a foreign body. This reminds us in a striking manner of what has been already expressed as follows: *Whenever a substance denser than the fluid which surrounds it, is found in the organic cavities, it becomes a determining cause of endosmosis.* We see now why foreign bodies denser than the blood produce hyperendosmosis or inflammation of the internal parts into which they have been introduced. The passage on the other hand of substances less dense than the blood into the tissue of the organs, so far from causing inflammation, will diminish its intensity, by diminishing the activity of endosmosis. Hence we know that the introduction of water into the animal frame is one of the most efficacious means for removing inflammation. In this way watery cataplasms on inflamed parts, bathing, and abundant aqueous drinks are so useful.

Water by being largely introduced has the double effect of diminishing the density of the fluids producing hyperendosmosis, and of replacing those which it expels by its constant introduction. The more the blood, the fluid external to the vesicles, is diluted, the more rapid will be the first action of endosmosis, but the more prompt also will be the renewal of the contents of the vesicles and of course the cessation of the hyperendosmosis. What was said of nutrition applies to the morbid state now under consideration; the changes are brought about in the *contained* matters,—the *containing* parts being foreign to the inflammation. It would seem then to be an alteration of the fluids which constitutes the diseases by hyperendosmosis; and accordingly it is by the fluids that contagious diseases are produced.

The part or organ in a state of hyperendosmosis is in a *turgid state*, and draws the fluids in abundance to it. The arteries are augmented in calibre and conduct the blood, directed to the inflamed spot, by *affluxion;* the veins coming from it are also dilated under the impulse of the blood, now sent to them from the part in greater abundance and with more force than usual. Thus we may consider an inflamed organ as at once the *object of affluxion* and the *origin of impulsion* for the blood. It is moreover, owing to the extension of the action of endosmosis, an organ of revulsion or derivation from other parts, which abandon a certain portion of their fluids. From this we learn the propriety of provoking an artificial hyperendosmosis, which shall be stronger if possible than the one primarily induced in a vital organ. In this way blisters and sometimes purgatives act. Leeching and cupping also operate by producing an affluxion in the part to which they are applied; because the vacuum which they produce tends to be filled by the endosmosis of the evacuated parts.

The author, after adverting to the empirical manner of treating inflammation and the necessity of taking physiology as our guide in medicine, lays down, according to his theory, the various means of combating morbid hyperendosmosis. Most of these have no claims to novelty, such as cupping, leeching, general bleeding, blisters, and watery drinks. The two first may relieve either by a direct evacuation of the altered fluids, if they are applied immediately over the part inflamed, or by derivation, if they are applied to a part adjoining, and there produce an affluxion. Theory here points out an additional therapeutical means, hitherto used very empirically. It consists in causing to be introduced by absorption, substances which, from their chemical nature, are calculated to produce a more energetic hyperendosmosis and the expulsion of the offending matter, that is, a renewal of the contents of the organic vesicles. In this way, Dr D. supposes, that mercury cures syphilis,—purgatives, dysentery,—vesication, erysipelas; that is to say, we substitute a cause of hyperendosmosis, comparatively safe, for one fraught with danger, as is the morbid cause.

The intensity of hyperendosmosis may be diminished by the introduction into the animal economy of certain agents capable of producing exosmosis. It is probably in this way that acids are serviceable in inflammation. Nothing is less suited, in Dr D.'s opinion, to the relief of this morbid state than the free ingestion of drinks, of gummy and extractive matters, or of a saccharine nature. If emollient cataplasms have a salutary effect when applied to inflamed parts, it is owing either to the water which they furnish for absorption, or to the exosmosis to which they give rise in virtue of their superior density. A poultice of flaxseed is well calculated to produce this latter effect, and ought consequently to determine towards the surface an electric current by which the fluids are directed from within outwards, whence will result the depletion of the hyperendosmosed part. This theory would seem to indicate on certain occasions the employment of baths made of fluids denser than those entering into the composition of our organs. Then would the skin exhale abundantly in place of absorbing, which last is the case where the body is immersed in pure water, or a liquid of the like inferior density.

Here we close our task, in which we have endeavoured, and we hope not without success, to give a succinct and unbroken narrative of the novel experiments and ingenious views of Dr DUTROCHET, as contained in his two works, on The Intimate Structure of Animals and Vegetables, and on The Immediate Agent of Vital Movement. We do not think it necessary to indulge in a laudatory strain respecting the merits of these productions: the lively interest we have experienced in their perusal is conclusive of our own feelings, and we are very sure that the reader of this review will be inclined to pay a similar practical eulogy to their meritorious and gifted author.

ARTICLE X.—*The Life of* EDWARD JENNER, M.D. LL.D. F.R.S. *Physician Extraordinary to the King, &c. &c. With Illustrations of his Doctrines and Selections from his Correspondence.* By JOHN BARON, M.D. F.R.S.

At the present moment, a good biographical memoir of the great discoverer of vaccination is most welcome. When there are found some, even of the ardent and earliest friends of this invaluable prophylactic, in doubt as to its real utility, it may be well to direct our attention once more to the history of its initial course. Without a present reference to the long and patient examination of the facts which triumphed over an able and well organized opposition at that time, we feel, in the perusal of the admirable portraiture of JENNER, drawn by his literate and intimate friend, a new confidence in the discovery, founded on the character of that acute, cautious and philosophic mind from which it emanated.

Every one who has read the sketches by Boswell must lament, that the biographer of "The Vaccinator" did not, like the delineator of Johnson, introduce us more frequently and fully into the recesses of the head and heart of JENNER, as they lay unfolded in the bosom of domestic life. Eminently a social being, his fine example should have been more amply developed; for we have too many incitements to the pursuit of public, ostentatious, and often unsatisfying virtues, whilst we need every inducement to the cultivation of those delightful, minor moral excellencies, that play around the soul in the seclusion of home, and which, leaving behind no satiety, and stimulating no vanity, are the sweetest and purest of human enjoyments. An intimate acquaintance, continued for fifteen years, together with the possession of the whole of the notes and correspondence of JENNER, should have afforded us a treat of the kind we speak of; but as it is, we are obliged to Dr BARON for what he has done, and hope that the pleasure received by us in the perusal of his memoir, will be felt by every one who reads it.

EDWARD JENNER was born at Berkeley in Gloucestershire, G. B. in 1749, and being the son of a respectable clergyman, received a sound education. In his childhood, he evinced that partiality for the study of natural history which so remarkably characterized his maturer years. The study of nature usually directs the attention of youth to the profession of medicine, and JENNER, following his bent and having completed his scholastic education, was instructed in the elements of surgery and pharmacy by Mr Ludlow, of Sodbury, near Bristol.

Attracted by the fame of JOHN HUNTER, JENNER became, at the age of twenty-one, his pupil and an inmate of his family. An acquaintance thus commenced, was matured into a solid friendship which lasted until the death of Mr HUNTER. An uninterrupted epistolary correspondence was continued between them, until the closing period of Mr HUNTER's life. Nearly all the letters of Mr HUNTER are preserved, and many of them published in the work of Dr BARON. HUNTER took no care of those of JENNER, and they are probably irrecoverably lost.

On the return of Captain Cook in 1771, JENNER, then a pupil of HUNTER, was employed to arrange and prepare most of the valuable specimens of natural history, collected by SIR JOSEPH BANKS. This duty led to the offer of the situation of naturalist to the next expedition which sailed in 1772. Having previously resolved to enter upon professional duty in his native home, he refused an offer particularly tempting to one of his taste and pursuits. At Berkeley, the toils of his vocation were sweetened by the elegant pursuits of natural history. His memoir on the cuckoo, one of the finest offerings to the science of ornithology, excited a greater sensation in England than any other work of that science in that age. Truth combined with novelty in a curious manner in this memoir. The cuckoo deposites its eggs in the nests of other birds, leaving its offspring to the care of foster parents. An instinct, curious on account of its singleness, teaches the young cuckoo to force from the nest the true offspring of the foster bird: whilst the latter is occasionally an unconcerned spectator of the destructive extrusion of its own unfledged flesh and blood. The paper detailing these facts was read before the Royal Society in 1788, and is contained in the "Transactions" of that year. In this year he was united in marriage to Miss Catharine Kingscote, a lady in every respect fitted to adorn and cheer the mansion of a man of taste and talent. Some years before, JENNER had been deeply disappointed in love, but whether Miss Kingscote or another had caused his distress does not appear. His disappointment excited a very characteristic expression of sympathy from JOHN HUNTER.

"I can easily conceive how you must feel; for you have two passions to cope with, viz. that of being disappointed in love, and that of being defeated; but both will wear out, perhaps the *first soonest.*

I own I was glad, when I heard that you were married to a woman of fortune; but let her go, never mind her, I shall employ you with hedgehogs, &c."

In 1792, wishing for exemption from a very fatiguing department of his profession, JENNER purchased a degree in medicine from St Andrews.

On the 14th of May 1796, a boy named James Phipps was inoculated with virus taken from the hand of a milkmaid, who had been infected by handling the diseased udders of her master's cows. This operation, the first *well attested* instance of the *intentional* production of a disease familiarly denominated cow-pox, was performed by Dr JENNER for the purpose of testing the truth of the *popular* opinion of the antivariolous powers of the vaccine disease. In a letter to his old and faithful friend EDWARD GARDNER, dated July 19th 1796, he thus expresses himself.

"You will be gratified in hearing that I have at length accomplished, *what I have been so long waiting for*, the passing of the vaccine virus from one human being to another by the ordinary mode of inoculation.

"A boy named James Phipps, &c............But now listen to the most delightful part of my story. The boy has since been inoculated for the small pox, which, as I ventured to predict, produced no effect. I shall now pursue my experiments with redoubled ardour."

It was in the spring of 1798 that he was again enabled to renew his experiments; for the disease had, until then, disappeared from the dairies. Seven inoculations were effected, one only of which failed. The failure took place in his own child, who was variolated afterwards to secure him from the danger of neighbouring small pox.

Armed with such conclusive experiments, and fortified with many cases, where protection from small pox was afforded by the accidental occurrence of *vaccinia*, Dr JENNER, after mature reflection, and frequent consultation with his friends, published his far-famed "Inquiry," and dedicated it to his friend, the celebrated Dr PARRY of Bath.

Prior to this momentous event, he, in April 1798, visited London, for the purpose of repeating his experiments before his medical friends in the metropolis. Who does not feel a difficulty in believing that a man of JENNER's high character, and known moral worth, should have spent three whole months in London without being able to find one person willing to submit to an operation, the usefulness of which had been so amply attested.

One eminent practitioner, Mr **Cline**, perhaps only partially moved by the statements of **Jenner**, deemed it necessary to resort to a kind of artifice to satisfy himself. Having under his care a case of disease of the hip for which issues were esteemed proper, he inserted some vaccine virus left him by **Jenner**, with a view to excite inflammation and form an ulcer. The subsequent ineffectual insertion of small pox matter brought complete conviction to the mind of Mr **Cline**, and he thus writes to Dr **Jenner** under date of 2d August 1798. "I think the substituting of cow-pox poison for the small pox promises to be one of the greatest improvements that has ever been made in medicine." The experiment of Mr **Cline** created a strong sensation in London. That gentleman endeavoured to propagate the infection but failed: and no farther experiments were made in London until the 20th of January 1799, when the virus was found in a dairy of the metropolis, and the practice of vaccination began to be fashionable in England.

In May 1799 Dr **De Carro** of Vienna vaccinated the child of a medical friend and soon after his own children. These were the first cases produced in continental Europe. Before the end of January 1801, fifteen hundred persons had been successfully vaccinated in Geneva, the native city of **De Carro**. To this gentleman's humanity Italy, Poland, Russia, Turkey, Greece, Persia, and British India stand indebted for the early introduction of the vaccine virus. In June 1802, the virus, in a good condition, reached the Company's establishment at Bombay; whence it soon spread over India.

It may readily be supposed that our shrewd and intelligent countrymen would be roused at the extraordinary and *useful* discovery of **Jenner**. On the 12th of March 1799, Dr **Waterhouse** of Boston gave to the American public a short account of the cow-pox; and his paper, published in the Columbian Sentinel, was headed "*Something curious in the medical line.*"

Shortly afterwards, with matter received from Dr **Haygarth** of Bath, Dr **Waterhouse** successfully vaccinated six of his own children; and. by a subsequent attempt at variolation, thoroughly satisfied himself of the prophylactic power of the new disease. It is to the credit of American physicians, that although, from the ignorance and cupidity of vaccinators, many errors were fallen into, yet no serious *public* opposition was made to the reception of the doctrine and practice of **JENNER**. President Adams gave early countenance to vaccination, and Mr Jefferson, with his sons-in-law, vaccinated nearly two hundred persons in his family and neighbourhood. Nearly two years after his first annunciation of this discovery to the American public, Dr WATERHOUSE writes thus to Dr JENNER. "24th April 1801. Could you believe that not a single case of the cow pox inoculation has yet occurred in Philadelphia? A young physician applied to me a few days since, from that city, for the infection: It seems that the leading physician there pronounces it too *beastly* and indelicate for polished society." On the 30th May 1801, he writes thus. "The inoculation finds no promoters in Philadelphia. I have had many letters lately on the subject from Virginia, &c." On the 5th November 1801, he says, "Had I not a kind of apostolic zeal, I should at times feel a little discouraged. The natives of America are skilful in bush-fighting." In another letter to JENNER dated April 8th 1802, he informs him, that Dr RUSH of Philadelphia "has come out full and strong in praise of the new inoculation, and has sent me a copy of an eloquent lecture on the blessings of the Jennerian discovery."

In December 1801, the discovery extended, partially at least, its benefits to the Indians of some North American tribes.

It is to be regretted that in the history of the progressive extension of vaccine inoculation, Dr BARON should have passed over in entire silence the most interesting and untiring efforts for its introduction into China. If its *three hundred millions* of inhabitants now enjoy this inestimable charm against the most loathsome and destructive of sublunary plagues, the credit is due to one single individual. Cautious, crafty, jealous of foreigners, and contemptuous towards strangers, the Chinese heard of the new disease with decided incredulity. It was not Chinese; it was therefore valueless, or if of value, unsuited to Chinese institutions. By stealth and by purchase, some of the meanest of the beggars were inoculated. Gradually and at a heavy expense, a sufficient number were thus protected to afford an exemplification of the benefits of the practice.

Had any death followed immediately, from any cause whatever, any case of vaccination, the inoculator would have probably fallen a victim to the bloody and indiscriminating laws against murder. under which term even accidental homicide is included. Against such opposition and in defiance of such obstacles, did ALEXANDER

PEARSON, chief surgeon of the British East India Company's establishment in China, fully force on the empire of China the salutary vaccine prophylactic. It is to be hoped that Dr BARON, in asserting the honest claims of Dr JENNER, has not suffered the name of PEARSON to drive him into that kind of injustice, against which he so strenuously labours to defend his hero. From Canton, the practice of vaccination, gradually but slowly received, reached the capital, and is now in almost universal repute.

A succinct but satisfactory history of the discovery and mode of vaccination, written by PEARSON and accompanied by illustrative plates, was by Sir George Staunton translated into the Chinese language, and widely distributed. There is a copy of this curious work in the Philadelphia library.

Notwithstanding the great novelty of the specific antidote, the strangeness of its origin, the mistakes of its friends, the blunders and the malignant opposition of its enemies, vaccination, in little more than six years from the date of JENNER's first publication, had passed into general use in almost every country on the face of the globe.

At the period at which we now write, when the usefulness of the vaccine disease is universally admitted, and the human passions, roused by interest and vanity, are slumbering in the tombs of those whom they once directed, we may be permitted to recal the scene of contention in which this blessing to our race entered on its splendid career of mercy. The lesson is a good one, and it cannot wound those who are now beyond the reach of praise or censure. Dr BARON, in presenting such a lesson, is able also to plead the imperfection of any biographical sketch, which should not exhibit the calmness and dignity of his hero, under an opposition, at once bitter and ignorant, hurtful and unjust.

Among the enemies of the Jennerian practice, stood pre-eminent a few of that antiquated class of pretenders to philosophy, who abhor the "test by experiment," and venture to pass sentence on things easily tried, without the least regard to the facts of the case. MOSELY, whose work on tropical diseases displays great learning, with much credulity and abundant arrogance, distinguished himself as the leader of this class of opponents. He wrote, as expressed by Dr BARON, "with all the bitterness and prejudice that generally attend on ignorance." He predicted an alteration in the human form:

"*Semibovemque virum, semivirumque bovem.*"

In reply to such charges, Mr Ring appealed to Moseley for an opinion as to the possible effects of beef-steaks, mutton-chops, and veal-cutlets: while Fermon tells him that, in his allegations against vaccination, "he had acted the part of the devil's advocate at a canonization, who was to say all the harm he could against the saint, in order that his life might be thoroughly scrutinized, and his merits appear the more conspicuous*."

The eminently successful progress of vaccination proves, that such men as Moseley and Ehrmann could but slightly retard the advancement of this great discovery; but while Jenner could not doubt the ultimate triumph of his antidote, he was made seriously to apprehend the loss of that fame and honour which were so justly his due. When his opponents could no longer conceal, either from themselves or others, the inestimable value of vaccination, they resolutely denied to Jenner the *credit* of the *discovery*.

Although from time to time hints against Jenner's claims were thrown out, and a vaccine institution was formed in London, in which he was to have no share, yet no direct and open opposition to his right to the discovery was made to any extent, until, after much labour and heavy expenditure, he offered a modest appeal to the generosity of his country. Before a committee of the House of Commons, appeared his old and subtle adversary Dr G. Pearson, who endeavoured to show that vaccine inoculation was practised by other persons, before the publication of Jenner's "Inquiry." In a subsequent examination, he claimed for himself and Dr Woodville the chief merit in the *establishment* of the vaccine inoculation. The reader has already seen the obstacles which Jenner had to surmount in obtaining favour for his new practice, even after he had made many successful experiments. When its value *was* perceived, and it had begun to heap honours on its promulger, all his toils, his persistency and his patience were forgotten, and England was ransacked for reports of cases of vaccination, which, but for his philosophy and his courage, would have remained unheeded and unknown. These idle rumours were readily believed by those who had long refused credence to the well attested experiments of an honourable and scientific physician.

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* "A learned divine of the church of England," says Dr Baron, "who preached a sermon against small pox inoculation, in London, 1722, announced it as no new art, inasmuch as Job, he asserted had been inoculated by the devil. Ehrmann of Frankfort took rather a bolder flight, and attempted to prove, from quotations of the prophetical parts of Scripture, and the writings of the fathers of the church, that the vaccine was nothing less than Antichrist."

The extent of Pearson and Woodville's services consisted in poisoning, with variolous virus, the uncontaminated cow-pox matter of Jenner, and mistaking the modified small pox thus created for the vaccine disease itself. This capital blunder greatly injured the early progress of vaccination.

Before the year 1770, and while in the shop of his preceptor Mr Ludlow, Jenner's attention was turned towards the popular opinion of the antivariolous powers of the cow-pox. To John Hunter he suggested it during his residence in his family, and, on his authority, Hunter continued in each succeeding year to mention it to his class. Pearson heard Mr Hunter on this subject, and *himself* gave the intelligence annually to *his* classes. According to Jenner's own testimony, he conceived the idea of conveying the vaccine disease by inoculation in 1775, and he mentioned it to his friend Gardner in 1780. Of the popular opinion respecting the prophylactic virtues of cow-pox, Jenner often spoke, in the medical societies at Alveston and Rodborough, and urged the prosecution of inquiries respecting it. "All his efforts were however ineffectual; his brethren were acquainted with the rumour, but they looked upon it as one of those vague notions from which no accurate or valuable information could be gathered, especially as most of them had met with cases in which those who were supposed to have had cow-pox had subsequently been affected with small pox. These discouragements, as will be seen more at large hereafter, did not suppress the ardour of Jenner's mind. He often recurred to the subject in these meetings; at length, it became so distasteful to his companions, that I have many times heard him declare that they threatened to expel him, if he continued to harass them with so unprofitable a subject*."

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* The least pardonable denial of the merit of Jenner as a discoverer was that made in 1821 by M. Husson in the article on vaccination in the Dict. des Sci. Med. It appears that in 1781, a year after Dr Jenner had expressed to his friend Gardner his desire to try vaccine inoculation, a protestant clergyman at Montpelier suggested the same idea to a physician from Bristol of the name of Pew, who then promised to mention it to his friend Dr Jenner, &c. M. Hus-

To JOHN HUNTER, JENNER was indebted for a disposition to experiment, so remarkable in that great surgeon and naturalist. In one of his letters to JENNER, who had attempted a theoretic solution of a difficulty, HUNTER writes thus. "I thank you for your experiment on the hedgehog; but why do you ask me a question by way of solving it? I think your solution is just; but why *think*,—why not try the experiment? Repeat the experiments on a hedgehog, and *they* will give you the solution."

In the true spirit of this sound philosophical injunction, JENNER pursued all his inquiries, and thus acquired much reputation in various walks of science, and finally overcame those obstacles, which for perhaps nearly one hundred years had prevented the general reception of cow-pox as a prophylactic, and confined the knowledge of the most valuable fact in medicine to a few dairy-farms in England.

So completely did he investigate this subject, that only one other person has been able to suggest any thing more on the subject. That person was Mr BRYCE of Edinburgh, who introduced the use of the crust for vaccinating, and published the curious fact that a vaccination made while a vaccine vesicle of the same person is in progress, will create a vesicle, which will run a more rapid course, and overtake the first, and then proceed along with it. This he proposed as a test of the true character of the disease, and of its having affected the whole system of the individual. The value of both suggestions is great. The use of the crust prevents the necessity of breaking the vesicle by which the regular progress of the protecting influence is left unimpeded or unaffected; while recent and most perplexing epidemics of a variolous nature enhance our esti-

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SON had studied his subject too well to be unacquainted with the evidence of JENNER's earlier attention to the subject. It also appears that JENNER had no friend named PEW, and that the only medical man of that name in his county, although engaged in a correspondence on vaccination in 1804, did not allude to the pretensions or conversation of M. RABAUT. It will also be noticed that it was at least fifteen years after this supposed communication, that JENNER put his theory to the test of experiment; during which time the Frenchman and his discovery remained in obscurity. The *conception* however was not the merit of JENNER, any more than it was the merit of Fulton respecting steam-boats. But it was his glory to have given a useful career to his discovery, and to have rendered the whole world his debtor for the *practical* knowledge of a preventive of the worst of physical evils.

mation of every measure likely to give certainty to our judgment in vaccination.

There is nothing in all which Dr BARON shows us of the very estimable JENNER, with one single exception, which does not place him in a highly respectable and most amiable position. That exception would not here be noticed, were it not thought useful to expose the greatest medical evil, whenever and wherever we find it.

Immediately on the publication of his excellent work on cowpox, Mr BRYCE sent a copy to JENNER, with a polite, modest, and highly complimentary letter. In it he gives JENNER the whole credit of the discovery and promulgation of *vaccinia*, and requests his opinion of his improvements. In his first answer, full of apologies *for its long delay*, JENNER merely says in reply to BRYCE's queries: "You have anticipated some few observations that I had noted down for my next number." Some time after he again addressed Mr BRYCE, and acknowledges the ingenuity of the "*test.*" "To all young vaccinators it cannot be too strongly enjoined. The *experienced* will determine from the character of the pustule." Here its chief value, as an evidence of a general impression, is overlooked. Again, "I put your crust into the hands of my friend RING, and he informed me yesterday that it had produced a good pustule. Experience now tells us that this is a good mode of sending the vaccine virus to distant parts." Here also the chief value of the suggestion is overlooked. JENNER lived to think more highly of BRYCE's crusts, and to enjoin the careful preservation of the integrity of the maturing vaccine vesicle.

The details of the private life of JENNER exhibit him in a most agreeable manner. Of the high character of his knowing and reflecting faculties, ample evidence is afforded in the history of his numerous scientific labours. In his profession, he stood in the foremost rank; and enjoyed the esteem and confidence of his medical brethren, and the whole community in which he resided. With all the accuracy and patience of HIPPOCRATES, he united much of the fire and fancy of AKENSIDE, the poetic playfulness and humour of COWPER, and the soft amiable sweetness of MEADE.—"The utmost vigilance of those who ignorantly assailed either his conduct or doctrines, has left no stain upon his name." In the height of a most pardonable exultation, he always felt his dependence on the Great Author of discoveries. To use his own words, "It is pleasant to recollect that these reflections always ended in devout acknowledgments to that Being, from whom this and *all other* mercies flow."

"Knowing that good company and good discourse were the sinews of virtue, he promoted them on all occasions. He was especially fond of music, and was a member of a catch club that met at Cam. He could also play on the violin and flute, and he was in the habit of forming select musical parties, where he occasionally was a performer.

"I have seen him in his latter years, after his renown had filled the world, and after the many cares attendant on vaccination had often weighed heavy upon him, shake them entirely off. He would then take up a humourous strain and sing one of his own ballads with all the mirth and gaiety of his youthful days.

"He had a particular dislike to cards, both because they interfered with a much more instructive employment of time, and often led to evils of a much more serious nature."

Medical Literature.

ARTICLE XI.—*Biographical Notice of* SAMUEL EMLEN, M.D. *By* CHARLES D. MEIGS, M.D.

Died on the 17th of April 1828, in the 39th year of his age, SAMUEL EMLEN, JR, M.D. Secretary of the College of Physicians of Philadelphia, and one of the Physicians to the Pennsylvania Hospital.

This is the third time, since the establishment of our Journal, that we have been called upon to record the decease of worthy and valuable members of our profession in this city. EWING was taken from the midst of us, just at the moment when his talents and virtues had begun to render his name familiar to the public ear as a rising and successful physician; the venerable Doctor GRIFITTS, as the close of a long life of devotion to the humane duties of medicine had crowned him with reverence and popular respect; and now, again, the irreproachable EMLEN, having slowly surmounted the first difficulties in the way to professional reputation, is, by an inscrutable decree of the Divine Providence, snatched from his family, his friends, and the art, which he seemed born to honour and advance by his industry, abilities, and exemplary life and conversation.

We have been commanded by the Kappa Lambda Society, of which he was a valued member, and officer, to prepare a sketch of his life for this number of its Journal; and in obeying this command, we hope that our feelings of personal attachment to him may not lead us to make any false estimate of his many virtues, or to overrate his acknowledged abilities. We sincerely desire at least to say nothing more than the simple truth in regard to our deceased member.

Such is the nature of our calling, that few physicians, whose lives have been short of half a century, have been able to furnish considerable materials for the pen of the biographer. **Baglivi**, **Saunders**, **Bichat**, and some others, furnish rare exceptions to the rule; and even their lives are rather to be read in the works they have left as bequests to posterity than in the events they witnessed, or the transactions in which they were personally engaged. The peaceful, quiet and unobtrusive tenor of even a good physician's life affords, for the most part, but little scope for details or description; since good sense, faithful discharge of duty, charitableness, inflexible integrity, christian piety, all that renders a man *integer vitæ scelerisque purus*, may be possessed in the highest degree without affording very striking features for such an article. What says the poet?

> Full many a gem of *purest ray serene*,
> The dark unfathom'd caves of ocean bear;
> Full many a flower is born to blush unseen,
> And waste its *sweetness* on the desert air.

But those gems that are concealed in the deep caves of the sea, and the modest flowers that blossom in desert places, are not less bright, or sweet, or admirable, because they are not seen of the world—and those men, whose excellent and rare worth might make them the admiration of the age, are not the less admirable, if, with a virtuous modesty or christian humility, they retire from the stark stare of the public, preferring to exercise in a quieter sphere their excellent virtues, until confirmed by time and experience, they with more confidence may claim the high places of honour and respect. Virtue, in their view, is not a meteor, to flash out brightly and straightway be seen no more; for bad men occasionally do good actions: but it is a steady and a shining light, whose beams are mild in the orient, and grow broader and brighter and more beneficent, until they attain a meridian perfection and excellency. Those men only who *pursue* wisdom, and grow daily in goodness, are entitled to our admiration and praise. If such persons do not become what is called great; it is because circumstances make men great, and not that great men create circumstances for themselves. Such genius and public virtue as are found to be common and almost trite in times of great political convulsions and revolutions, ought not to be considered as rare or uncommon among men; for multitudes of persons pass their lives in vile trades, or squander their time in humble pursuits, who only require favourable circumstances to enable them to advance science, adorn the arts, or fill the rolls of fame with the history of glorious actions: the same is true of physicians, whose greatness oftener depends on contingencies than on their own pre-eminent qualifications. The occurrence of terrific epidemics, or fortunate appointments to public stations, are frequently the causes which lift men far above their equals in talent or worth.

After all it might be asked, what is it that makes men worthy of imitation when living, and of reverence and regret when dead? He only is admirable who begins life with unchangeable resolves to discharge his relative, social, and religious duties, and who, in the course of that life, brings constantly up to the mark in performance, what he had aimed at as the prize in promise. The common fault is that men go on by a sort of rule of chance-medley, have no fixed and predetermined objects or motives, and yielding to the impulse of events, are ever swaying up and down, and come therefore to nothing good. A man may begin with a general resolution that he will lead a moral life; but he is not half so apt to escape the snares of temptation, as he, who firmly determines to eradicate from his soul the particular seeds of pride, envy, malice, avarice, &c. The very definiteness of the plan ensures its fulfilment.

Cicero says of the wise man, that " amputata circumcisaque inanitate omni et errore, naturæ finibus contentus, sine aegritudine possit, et sine metu vivere :" to be restrained within the "naturæ finibus" is to possess moderation—it is to know duty and to do it.

In the death of Dr EMLEN we have suffered the loss of a man who understood well, and discharged in a high degree, his professional, social, and religious obligations—who had forced his way by sheer merit, without an iota of false pretences or shrewd policy, into the public favour.

Dr EMLEN was born in Chester county, state of Pennsylvania, on the 6th of March 1789. As springing from one of the oldest and most respectable families of the society of Friends, he received of course, in his early education, all the advantages which their strict example and sedulous inculcation of good morals could bestow. His education was chiefly English; but as it was carefully superintended, he laid in it a solid foundation of knowledge, on which he afterwards erected a considerable structure of various and available information. The flash and gewgaw of education were never very desirable nor pleasing in his eyes, inasmuch as he knew them to be unessentials in managing the solid and stern concerns of life; and herein he conformed to the practice of the religious body of which he was a member, who, though they despise not many of the elegant pursuits of literature, and often combine in a high degree the agreeable with the useful, are more given to the latter than to the former. Dr **Emlen's** acquirements were more solid than specious, and produced in him those excellent fruits which have caused his death to be so much regretted.

In the year 1808, having resolved to devote himself to the profession of medicine, he placed himself as house pupil with Dr **Parrish** of this city, and, under his roof, and with his example constantly before him, made rapid progress in his studies, to which, by the testimony of his teacher, he absolutely devoted himself.

Under the roof of Dr **Parrish** and as a member of his family, Dr **Emlen** passed four years, during which having attended the lectures delivered in the University by the professors **Rush**, **Wistar**, **Barton**, **Physick**, **James** and **Coxe**, he graduated M.D., and in June 1812 embarked at New York for England.

Arrived at London in the month of July, he placed himself in the vicinity of one of the great hospitals, where he sedulously endeavoured to acquire the greatest amount of practical and surgical knowledge. Attendance on hospital practice, on lectures by the celebrated individuals whose reputation had attracted him thither, conversation with celebrated men, to the houses of many of whom he had free and familiar access, and visits to objects which interest the man of science or the philanthropist, kept his mind on the stretch; and he accumulated a large stock of information, of which he noted down the heads in his journal, which we have perused with great satisfaction, as affording evidence of the diligence with which he employed himself even at that period.

The declaration of war by the United States against Great Britain, which reached London soon after his arrival, placed no obstacles in the way of his studies while in the metropolis. The detention it occasioned gave him an opportunity, however, of making an extensive tour through England, Ireland, and Scotland, the history of which is detailed with considerable naiveté in his journal. At length the obstacles to his visit to Paris were removed, and after a residence of fourteen months in the island, he reached that city about the time of the Emperor's return from Leipzig.

His stay in London and his frequent access to the society of the most eminent physicians, surgeons and lecturers had increased his stock of knowledge, while the elegant society in which he moved, although it never abolished the gravity of his carriage, or the serious and sententious style of his conversation, imparted nevertheless to his manners that urbane cast, which is far more estimable and trustworthy than the false and heartless elegance of mere fashionable intercourse. They were marked by the gentleness, self possession, and confidence which belong to the gentleman.

In Paris, though daily attracted by the extraordinary events of that wonderful period of history, Dr EMLEN continued to attend mainly to the objects of his visit. The battles fought in the vicinity filled the hospitals with soldiers suffering under every species of military accidents, which he carefully studied.

As we have no events of his history while in France, demanding a particular relation, we need here only state that after the surrender of the French capital he returned to London in June, from whence he proceeded to Holland, and came home in the corvette John Adams as the bearer of despatches to the Government, after an absence of nearly two years and a half.

Soon after his arrival he commenced the practice of physic, and was elected one of the physicians to the Philadelphia Dispensary; an excellent school of practice, through which most of the eminent practitioners here have passed.

In 1819 he resigned this station, in consequence of increasing occupations; soon after which he was elected to be one of the managers, and finally after the death of his revered friend Dr GRIFFITS became secretary to that charity.

During the year 1819, when the yellow fever prevailed along the water margin of the city, Dr EMLEN was secretary to the Board of Health, and made those observations of which the fruit is to be found in his valuable paper on yellow fever published in the last number of this Journal.

As member of the Board of Guardians of the Poor, as physician to the Magdalen Asylum, the Orphan Asylum, and the Friends' Asylum for the Insane, he established broadly and deeply the foundations of a reputation which tended daily to raise him in the public esteem.

He was an efficient and respected member of the Kappa Lambda Society; and the Journal of that Association is much indebted to him for the usefulness and reputation it has attained. He succeeded Dr GRIFFITS as secretary to the College of Physicians, and to his zeal is undoubtedly owing much of the renewed activity and efficiency which marks the present course of that institution.

In 1825 he was elected one of the physicians to the Pennsylvania

Hospital, an office to which he was annually re-elected, a sufficient proof of the assiduity and ability with which he discharged the functions of that honourable and very responsible situation.

This excellent man sat not down contented with the discharge of his merely professional duties. He had acquired very solemn impressions of the magnitude of the evils which the vice of drunkenness has brought on the country, and few persons, although much attention has been given to it by some of the foremost men of the time, had accumulated more of statistic knowledge on the point than himself. In the organization of the Pennsylvania Society for Discouraging the Use of Ardent Spirits, as well as in its administration as Manager, he took a very active and discreet part.

Dr Emlen's private business occupied a very large share of his time. It had augmented rapidly during the last few years of his life; so that, with his public and private affairs, he had little leisure for visits of ceremony, or for any waste of that time which in his eyes was so valuable.

We have said that a physician's life for the most part furnishes few materials for the biographer: and yet, could we trace step by step so good a man's walk; behold him as he carries into the haunts of sickness, pain, and despair, the healing influences of his function; see him at the bed side of the declining and dying, calling the careless or amazed senses to a just consideration of the duties of the creature towards the creator; could we feel each throb of that sympathizing heart, or sum up the numerous almsgivings of that ever open hand,—what more should we need of events, to grace his memory or make his history interesting? The mad ambition of a conqueror may drive his headlong squadrons from the Granicus to the Indus over prostrate rights and bleeding bodies—may gather trophies, and wear them; but in the eye of sober reason and reflection, such are not half so lovely, so good, so beneficial in their day, as the ceaseless and noiseless triumphs of a good physician, whose christianity adorns and at the same time is illustrated by a blameless life.

Dr Emlen was a christian: those who knew him will appreciate this praise. His religious diary, commenced in 1823, was continued up to the day preceding the invasion of his last illness, and contains a faithful, candid transcript of his feelings and views concerning the immortal hopes and desires that he experienced. It affords the evidence of that strained anxiety for improvement in morals, which he seems to have made a regular part of his plan, and of which he never lost sight in his conduct or conversation. We ought to remark that, during his absence in Europe, he had been so much in the world, as to have omitted some degree of that rigid observance of *plainness* that marks the dress and manners of Friends; but, as his mind began to be more and more impressed with his religious obligations, he became more conformed to the practice of his society. His diary has an entry relating to the change he had effected in dress and demeanour, which was made for the purpose of enabling him, with less embarrassment or inconsistency, to live up to the requisitions of his conscience.

He quotes an observation, " we become christians more speedily by changing that which is within than that which is without ;" and subjoins, that in order to enable him to restrain his conduct with less hesitation within the bounds of the cross, and feel as though he had no plea for appearing to act as a worldly man, he had assumed that plainness of style which he doubtless considered as customary and useful aids in the christian warfare.

In the year 1819, he married Beulah Valentine, who was like himself a member of the Friends' Society. In the tender relations which this union produced, he found the purest sources of happiness. To his children he bore an affection that might be called passionate. We presume to say that the fire of parental love glowed in his breast with redoubled intenseness, perhaps because of the habitual restraint under which he was accustomed to hold his passions : how lamentable must have seemed the stroke which divided him in this world from that care and watchfulness over his children, which appeared to be, for him, the best part of existence. Nevertheless, in committing his family, as he did, on his death-bed, to the providential care of his Maker, he seemed to have acquired a calmness and submission that permitted no murmuring word to escape his lips, nor allowed of one sign of impatience or wilfulness, to express his unwillingness to meet that fate for which he was prepared by a blameless life.

Dr Emlen merited the praise due to wisdom ; and it is of such that Cicero says,

Rectius enim appellabitur rex quam Tarquinius, qui nec se nec suos regere potuit : rectius magister populi (is enim est dictator) quam Sulla, qui trium pestiferorum vitiorum, luxuriae, avaritiae, crudelitatis, magister fuit : rectius dives quam Crassus, qui, nisi eguisset, numquam Euphratem nulla belli causa transire voluisset. Recte ejus omnia dicentur qui scit uti solus omnibus. Recte etiam pulcer appellabitur: animi enim lineamenta sunt pulchriora quam corporis. Recte solus liber, nec dominationi cujusquam parens, neque obediens cupiditati. Recte invictus, cujus etiam si corpus constringatur, animo tamen vincula injici nulla possint.

ARTICLE XII.—*Biographical Notice of* HENRY M. READ, M.D. *By* D. THEODORE COXE, M.D.

The short career allotted to the late Dr READ will account for the comparatively brief obituary herewith presented. The laudable custom of the Kappa Lambda Society, however, of noticing the decease of its members, not being intended solely to commemorate age and eminence, is here complied with—and so long as this Society exists, may it never cease to offer an humble tribute of affectionate respect and remembrance to its junior and undistinguished members, as well as the eulogistic and laudatory oration upon its senior and eminent ones. The brotherly communion among its members, which Dr READ so highly prized, was indeed of short duration; for, just as an increased acquaintance was revealing to his professional brethren the merits which his diffidence and reserve had at first partially concealed—the arms of friendship were exchanged for those of death!

HENRY MEREDITH READ was born in Philadelphia, October 31st 1802, and there received the rudiments of his education. He early showed a fondness for books, and read with care and profit. His disposition was kind and affectionate, and characterized by a freedom from all jealousy and harshness. These qualities, joined to his equable temper and warm feelings, soon gained him the attachment of his young associates, notwithstanding his naturally retired character. An inclination to abstract himself from the world increased with his years, and eventually led to the solid improvement of his mind.

Early placed for his education in the University of Pennsylvania, he there pursued his studies for some time; but his friends, appreciating his capacity for literary attainments, and thinking a more retired spot would suit him better, transferred him, at the age of fourteen, to a private school at Trenton, New Jersey, under the superintendence of the Rev. Mr Filer, whose object was to instruct a limited number of pupils, and thus do them more ample justice. The change was beneficial, and young READ made satisfactory progress. Among the most diligent, he rose early and continued late, that he might be prepared, and give satisfaction to his instructor.

The course of studies at this seminary was particularly calculated to prepare young men for Princeton College, and thither accordingly READ went at the end of two years, being, in May 1818, admitted into its junior class. Here he remained two years, diligent in his studies and exemplary in his deportment, and finally received his degree of Bachelor of Arts, together with one of the honours of his class. His unwearied application during his four years at Trenton and Princeton is thought by his friends to have first weakened his constitution, and laid the foundation of the disease which eventually terminated his career.

On his return to Philadelphia, his first object was to select a profession, and after wavering for some time, he at length fixed on that of medicine, being thereto much influenced by his grandmother Mrs Meredith, daughter of the late DR CADWALADER, and who had in many respects supplied the place of his mother, whom he had lost some time before. He accordingly commenced his studies under Dr CHAPMAN, and attended the lectures of the professors in the University of Pennsylvania.

In April 1822, he became a resident student of the Alms House of this city, and assiduously devoted himself to the duties of his station, which he discharged to the satisfaction of all concerned. At the end of a year he left it, with the regrets of his associates, and the grateful recollections of its inmates.

Having completed the usual preparatory routine, and satisfactorily acquitted himself in his examination before the medical professors, he received, in the spring of 1823, his degree of Doctor of Medicine, taking for the subject of his thesis "The treatment of Mania à Potu."

Shortly after commencing his professional career in this city, he was appointed one of the physicians to the guardians of the poor, and became a member of the Medical and Kappa Lambda Societies of Philadelphia.

In the fall of 1827, an advantageous situation for his professional pursuits offering itself, he removed to Mount Moriah, near the boundary line between Pike and Northampton counties in this state. He here established himself perfectly to his satisfaction, and soon acquired the esteem and regard of all who became acquainted with him either privately or professionally. The future was now bright before him. But alas! " how many of our most certain and brilliant prospects fade before the blasts of earthly misfortune! hope rises before us like a meridian sun, throwing a halo of glittering splendour o'er our path—but ere the buds of promise open, they are withered and destroyed by the reckless hand of death!"

Thus it was with Dr **Read**. In the midst of his usefulness, and at the threshold of life, he was attacked by an affection of the liver, which baffled his own skill and that of his friend and physician Dr **Rosa**; and in a few weeks, at a distance from his family, he breathed his last: thus adding another solemn memento to the mournful catalogue of afflicting dispensations to which our transitory scene is subject.

Bibliographical Notices.

ARTICLE XIII.—*Traité Général d'Anatomie Comparée. Par* J.-F. MECKEL. *Traduit de l'Allemand et augmenté de Notes, par* MM. RIESTER, *et* ALPHONSE SANSON, *Docteur en Chirurgie de la Faculté de Paris. Précédé d'une Lettre de l'Auteur. Tome* 1er. *Paris*, 1828. *Pp*. 613.

A General Treatise on Comparative Anatomy. Translated from the German by MM. RIESTER *and* SANSON. *Vol. I.*

SUCH is the title of a work which will, we are convinced, form another monument of fame to the already highly distinguished author. Having previously given to the world the result of his researches in human, pathological, and systematic comparative anatomy, the learned professor of Halle brings before the public the present production, with every claim to the respect and gratitude of his fellow labourers in this field of science. His English and American readers must feel themselves under many obligations to his French translators, not only for their accurate version, but for the judicious observations contained in their valuable notes. We view the successful prosecution of this valuable department of science by our contemporaries of the continent of Europe, with mingled feelings of respect and mortification. As Americans, and members of a great and enlightened republic, our pride is naturally wounded by a conviction of the total apathy with which is regarded the teaching of comparative anatomy to our medical students. The science indeed is as totally neglected in our medical universities as if it constituted no branch of medical, not to say of *universal*, knowledge. Perhaps this neglect may be in some degree attributable to the aversion of some of the learned members of our profession to acknowledge the importance of a science, to the principles of which they have themselves devoted scarcely any attention; but let them reflect, for a moment, on the difficulties which they have encountered in the intricate paths of even elementary knowledge; let them but view the endless chain of exploded theories of respiration, digestion, secretion, the generation of animal heat, &c. which but for the reprehensible ignorance of their authors, of comparative anatomy and general physiology, would never have found existence. In fine, when we consider that all we do really know of the functions of the human body is directly or indirectly due to the study of comparative anatomy, including general physiology, we will be forced to acknowledge.

with Haller, that medicine is more indebted to this than to any other branch of knowledge.

The labours of BLUMENBACH, MECKEL, RUDOLPHI, TIEDEMANN, RATHKE', TREVIRANUS, &c. in Germany, of CUVIER, BLAINVILLE, GEOFFROY, and many others, in France, and of LAWRENCE, M'CARTNEY and HOME, in England, among the modern authors who have successfully cultivated *comparative anatomy*, justify the assertion, that " a knowledge of the facts to be derived from its study is absolutely indispensable to the philosophical and successful prosecution of the study of human physiology, pathology, and the doctrines of hygieine." We cannot but cherish the hope that the day is not far distant, when America, who has so liberally repaid her European debt in some other departments of science, will also soon assert her claims in this.

The present work of M. MECKEL should be placed in the hands of every student of comparative anatomy, after he has made himself acquainted with Baron CUVIER's Leçons d'Anatomie Comparée, which may be considered as an epitome of the whole science. Our author's work abounds in enlightened general views, and contains numerous facts which, in their origin and promulgation, are peculiarly his own. Among the first, must be classed M. MECKEL's proposition to unite all the varieties, in the corollaries of a common law, which he terms "*the Law of Variety.*"

The author observes in a natural chain of reasoning;—" after having endeavoured to develop these laws and conditions in memoirs particularly devoted to the subject, I have thought it necessary to collect all the subordinate circumstances which the animal kingdom presents, when viewed in general, into two principal points, viz.: *variety*, and *unity* or *analogy*.

" It is, in fact, under these two points of view that we ought always to consider the animal form; if we expect that the exposition of this form should not consist merely in the enumeration of differences and individualities, or in following specious resemblances, which too frequently exist only in the minds of those who search for them."

We think we detect some allusion, by the author of these remarks, to the peculiar views and speculative philosophy, disclosed in the " *Philosophie Anatomique*" of M. GEOFFROY ST HILAIRE ; many of whose ideas have been far from appearing to us as philosophical as they are novel. For example, we are taught that the cranium of animals, as generally considered, has no existence, but that the bones of which it is constructed are rudiments and modifications of the vertebræ. Again, he relates that the cloaca of oviparous animals is nothing more than the bladder of mammalia, with the rectum opening into its fundus; through which must pass not only the fæces, urine, &c. but also the eggs of the female; though the author neglects to inform us, how, in this case, we are to arrange those animals which possess both cloaca and urinary bladder distinct, many examples of which will be furnished him by his intimate knowledge of the anatomy of the amphibia, &c. We have also, occasionally, been under the necessity of differing with M. GEOFFROY in matters of fact, as well as of opinion. We have seen it published, as one of his discoveries, that he has been enabled to detect the existence of the umbilicus in the fœtal opossum. We have been unable to detect any rudiments of the organization alluded to, in the fœtal opossums which we have dissected.

Returning to the work of M. MECKEL, we detect no views merely speculative, much less any unphilosophical deductions. In treating of special anatomy, more particularly, he has offered many new observations and descriptions. We must not pass "sub silentio" his interesting discovery of the *mammæ* of the ornithorhynchus, or duck-billed quadruped, of New Holland; an animal hitherto supposed to be oviparous. The animated discussions to which this discovery has given rise in Europe render the subject highly important. If M. MECKEL's opinion should prove correct, it will destroy the intermediate class of "*Monotremata*," in which M. GEOFFROY has arranged the ornithorhynchus and echidna, and replace these in the situation which they formerly held, at the end of the class Mammalia; besides annulling our belief in a circumstance altogether astonishing, and hitherto without analogy; viz. the oviparous birth of animals mammiferous in all the grand points of their organization. But if we can no longer doubt the existence of mammæ in these paradoxical creatures, it yet remains to be proved that the organs really exist otherwise than as mere rudiments. Indeed, how is it possible for a young animal with the bill of a duck to live by sucking? To us it appears that it would require something more than "a great sensibility of the lips" to enable it to suck a mamma confessedly destitute of a nipple. The author states a fact, in his opinion, confirmatory of the nature of the glands in question; viz. that these organs have no existence in the male. This circumstance would incline us to infer directly the reverse; as in all animals truly mammiferous, these glands, particularly the nipples, equally exist in both sexes. We are strongly inclined to the opinion that, in the ornithorhynchus the mammæ exist, as these organs do in the male of all quadrupeds, viz. as rudiments, or marks of the "mechanical" laws on which the body is constructed. If we rightly remember, the eggs of the animal have recently been received in Europe.

The order MONOTREMATA must still continue to present us with that truly singular phenomenon, the existence of oviparous mammalia.

M. MECKEL, after having determined the various composite organs which occur in the construction of animals, classes the animal kingdom according to the complication of its organization; placing the polypus first in the scale, as offering the greatest simplicity of structure. The first volume contains four chapters: 1. On the laws of formation. 2. On the most important conditions of the animal form. 3. On the law of variety. 4. On the law of reduction to a common type. We anxiously wait the arrival of the future volumes. H.

ARTICLE XIV.—*A Practical Treatise on Poisons and Asphyxies, adapted to General Use, followed by directions for the Treatment of Burns, and for the Distinction of real from apparent Death. By* M. P. ORFILA, *Professor of Medical Chemistry, &c. &c. Translated from the French, with notes and additions, by* J. G. STEVENSON, M.D. *Boston,* 1826. *Pp.* 230, 12*mo.*

Our notice of this translation has been delayed by accidental causes. This we regret, as we conceive it well worthy of the widest and most rapid dissemination. That by Dr WILLIAM PRICE has not, we believe, been republished in

America. The reputation of M. ORFILA, and in particular, the usefulness of his "Secours a donner aux personnes empoisonnées et asphyxiées," are too well established to require being made known through the present vehicle. The learned author is perhaps that individual, in a scientific nation, who has in the completest manner applied science to this subject. The giving of medico-legal testimony in France is not, as with us, referred to the nearest medical man, however little his previous studies and habits may have qualified him for the delicate task: there are, in the departments, *juries*, as they are called, appointed by the government, whose especial duty it is to furnish it whenever required by law. The preparation requisite for serving in such a commission necessarily requires a considerable amount of labour; and whatever objections we may have to the appropriation of a term, taken from the more republican practice of our Saxon ancestors, to a body selected by authority, there can be no doubt that individuals so chosen will be better informed upon the subject than practitioners at large. At the head of these institutions, the French government has placed the author of the present work, and never has an appointment been more fully seconded by public opinion, even including nations remote from the scene of his employments. The commanding rank of M. ORFILA leaves him at once perhaps without a rival; and we are bound to admire the energy of character which leads him to persevere in his useful industry, still making new discoveries and improvements, long after the fear of competition is extinct, and the love of utility which induces him, at the height of science, to prepare the humble and popular manual before us. "Daily experience proves that diseases of this nature seldom terminate fatally, except in consequence of the ignorance of those who surround the sick at the commencement of the affection." P. 1. If this be correct, of what infinite importance is it then that books of this kind should be in the hands, not only of medical men, but of all those who, by character or station, possess that influence over the minds of others which enables or calls upon them to give directions in cases of alarm and emergency. Clergymen, magistrates, and private individuals distinguished for their force of character and general information, are frequently looked up to, in the absence of physicians, for their advice in cases of difficulty and terror. To such, therefore, in addition to the profession, is this volume addressed, in the original; and we certainly conceive the principle to be at least equally applicable in this country.

The style is popular and brief; the descriptions of symptoms, and the directions for procedure, as short as possible, divested of every unnecessary word, and generally of every unnecessary remedy. M. ORFILA has, we think very properly, omitted all antidotes of doubtful or dangerous use, or which are difficult to obtain; and confined himself mainly to a few simple and safe articles, always easy of access. Of these, the principal are, the *white of egg*, *milk*, *common salt*, *vinegar*, *lemon-juice*, *soap*, *and gall-nuts*. The person, physician or otherwise, who refers to a practical volume for instructions in a sudden and alarming case, does not wish to find himself bewildered by long chains of investigations, or a superabundant choice of remedies, with regard to whose usefulness he cannot make up his mind. What he needs is prompt and explicit directions which may enable him to act with decision and dispatch; and, if he employ the volume before us, he will not be disappointed.

The author begins by general considerations on poisoning, its symptoms and treatment. He then proceeds to classify poisons; dividing them into 1, The irritating; 2, The narcotic; 3, The Narcotico-acrid; and 4, The Septic or Putrid Poisons, a division which he composes of venomous bites or stings, animals injurious when eaten, and the matter of malignant pustule and hydrophobia. Under each of these classes, as much is said of their common characters as the case admits; thus saving the useless labour of repetition. The reader who wishes a complete account of the symptoms indicating any poison, must, therefore, peruse the paragraphs at the head of the chapter. This arrangement greatly contributes to the brevity of the book, of which the reading part is comprised in 230 duodecimo pages. Next follow the symptoms and treatment of asphyxies, the means of making the important discrimination between asphyxia and apoplexy in new born infants, and those of distinguishing real from apparent death, so necessary to guide us in our endeavours to recover the drowned, &c.; and the whole winds up with practical directions on the treatment of burns, a subject of great interest, and for want of a knowledge of which great sufferings are often incurred. In running over this, we feel called upon to express our satisfaction that the emollient and sedative plan is here recommended, instead of spirit of turpentine, alcohol, ammonia, heat, and the other mercies of the *homœopathical* treatment. We appeal, for the justification of our remark, to the memory of any one who has been so unfortunate as to experience a comparative trial of the two modes of treatment in his own person, and who has had, at the time, vitality enough to feel their effects. M. O., however, still retains lime-water, and admits the application of acetate of lead to denuded parts; so that those fond of witnessing contortions in the patient's countenance may still enjoy that satisfaction under the authority of this distinguished name.

In an appendix, brief directions are given respecting the precautions necessary in opening dead bodies with a medico-legal view, in drawing up reports, &c. which are much needed. They are extracted from a little code of instructions by professor CHAUSSIER.

We have not room for many more remarks, and will only add that the translation is well, neatly, and remarkably clearly executed, not excluding an occasional oversight; and that the copy in our hands is extremely neat in paper, typography, &c. From the fact of its being made in America, we derive an intrinsic recommendation to the book itself; as such a one, on equal terms, may always be presumed to be better adapted to our wants; and, besides, it is desirable to promote the existence, among ourselves, of a class of labourers, whose employment is to enrich our libraries with judicious selections from the treasures of European science.

Article XV.—*Versuch einer kritischen Geschichte der bei Vaccinirten beobachteten Menschenblattern, nebst Untersuchungen über die Natur, die Ursachen und die Verhütung dieser Krankheit. Von Dr* Adolph Friedrich Luders, *Köneg. Dän. Physicus zu Eckernförde im Herzoth. Schleswig, Mitgliede der Königl. mediz. Gesellschaft in Kopenhagen.*

An Essay towards a critical history of the Small Pox, as it occurs subsequent to Vaccination; together with an Inquiry into its Nature, Causes, and Mode of Prevention. By Dr A. F. Luders, *Health Officer for Eckenförde in the Duchy of Silesia, Fellow of the Royal Med. Soc. of Copenhagen. Altona,* 1824. *Pp.* 226, 8*vo.*

A copy of this work was some time since presented by the author to the library of the Philadelphia Medical Society. Though we were much pleased with its contents when we first hastily perused it, and laid it upon our table for a more minute examination, we have not until now had time to take it up, with the view of presenting our readers with a sketch of the experience of Dr Luders on the important subjects of which it treats. At a period, however, when every thing in relation to small pox and vaccination is undergoing the investigation of the medical profession in this city, we conceive that we should not be fulfilling our duties as editors, were we longer to delay some account of Dr L.'s interesting publication.

It is very generally known, that soon after the promulgation of the discovery of vaccination by Dr Jenner, the practice was introduced into Germany, and every measure taken by the several local authorities to extend its protecting powers, in the most effectual manner, to every individual within their jurisdiction. So successful were these efforts, that for a period nearly of twenty years, the small pox was unknown in many extensive districts, and it was even believed that this destroyer of human life had been effectually expelled from that portion of the globe. In 1822, however, and subsequently, variola appeared as an epidemic in Germany, and the other northern countries of Europe, and to the surprise of all, affected many who were supposed to have been fully protected from its influence by vaccination.

During these visitations, Dr L. held the important situation of *Physickus*, or health officer, for Eckenförde, in the Dutchy of Silesia, the very duties of which office obliged him to see and study the disease to an extent, far beyond what is in the power, generally speaking, of any private practitioner. The experience thus obtained, has enabled the Doctor to bear ample testimony to the efficacy of vaccination as a means of disarming small pox of nearly all its terrors. So far from being weakened in his confidence as to its preservative powers, he conceives that he has reason to maintain, that an individual whose system has once been effectually placed under the influence of genuine vaccine virus, is forever exempted from an attack of small pox, even in a modified form. In support of this opinion, he refers to the fact, that of 233,939 persons vaccinated, in Holstein, between the years 1801 and 1822, but two, as late as 1821, had been affected subsequently with small pox in any form; and that in Denmark, among 447,605 vaccinated individuals, but one case of modified small pox had occurred*. Other countries, he adds, as France, Sweden, Prussia and Austria, " present an equally favourable result in proof of the entire protection afforded by the genuine vaccine disease."

The reason why small pox, subsequent to vaccination, has so frequently and extensively occurred in Great Britain and other countries, Dr L. attributes to one or more of the following causes. 1. The use of vaccine virus of an inferior quality, only calculated to produce a vesicle of a modified character. The inferiority of the virus may, he conceives, be owing to its having been procured from a vesicle on the teat of the cow, other than genuine vaccine; from the modified vesicle which forms in those who have been previously vaccinated, or have experienced small pox either natural or by inoculation; from the modified vesicle frequently occurring from even the genuine virus in the less susceptible constitution of adults; from the modified vesicle formed even in children of cachectic habits, or who are labouring at the time, under some cutaneous disease; from vesicles previously ruptured, or of too long standing; or from a serous vesicle. The lymph or virus employed may also have deteriorated from having been too long kept; from exposure to damp, acid or strong scented vapours, &c. or, finally, the inferiority of the virus may arise from its gradual deterioration, in consequence of its having been successively transmitted, in the reproduction of the disease, through too great a number of individuals. 2. In communicating the vaccine disease, the formation of too few vesicles. 3. In cases where there has been but one vesicle, its having been rubbed, or otherwise accidentally ruptured, or its having been intentionally opened to obtain lymph. 4. The vaccine vesicle having undergone a modification in consequence of the coexistence of some constitutional affection, as scrofula, venereal, &c., of cutaneous eruptions, or of a state of convalescence from any severe or debilitating disease. 5. The vesicle having been modified either by external remedies, as camphor, and the ointments of mercury or lead, employed during its progress, or by internal remedies, as mercury, nitre, long continued purging, &c.

Dr L. conceives, that the modifications produced by the foregoing causes very generally escape the notice of the careless observer, more particularly, as even in the various public institutions, too little care is taken to watch attentively the progress of the vesicle subsequent to vaccination, in order to be assured of its regular progress and genuine character.

Dr L. affirms that in Germany, the vesicles produced by the matter resulting from the successive reproduction of the disease, from the virus originally sent to that country from England, are marked by an areola of less extent, and a less degree of irritation of the general system, than those produced by matter from the cow, after only three or four reproductions in the human system; and that while modified vaccine vesicles were, at the time he wrote, of more common occurrence, large, unequivocally genuine vesicles were less frequently met with than when he commenced vaccinating.

Dr L. is no advocate for revaccination; he does not believe that the preventive or modifying powers of vaccination are in any degree weakened by time. He has been well assured that modified small pox has occurred at every period, from three weeks to twenty years, subsequent to reputed vaccination, without the character of the disease being marked by any greater or less degree of severity, in consequence of the greater or less interval which had elapsed since vaccination had been performed.

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* In 1809, there were admitted into the Hospital at Copenhagen, 139 small pox patients, of whom one was supposed to have been vaccinated, and three had previously had the small pox. Page 74.

Dr L. examines the question of the identity of the modified form of small pox and chicken pox, and adduces many facts and arguments to prove that they are distinct diseases.

Dr LUDERS is undoubtedly a man of talents and observation. His entire work is deserving of an attentive perusal on the part of every practitioner acquainted with the language in which it is written. Besides the important collection of facts and observations it contains, the result of the author's own experience, in the two first chapters will be found a succinct account of the several varioloid epidemics which had occurred down to the period of its publication, with a copious reference to the various sources from which the materials for this account are derived, whether separate treatises, or essays contained in the medical journals of Europe and America. φ. κ.

ARTICLE XVI.—*An Essay on the Structure, Functions, and Diseases of the Nervous System; defended before the Medical Faculty of the University of Pennsylvania, March* 14, 1828, *for the Degree of Doctor of Medicine. By* JOHN R. W. DUNBAR, A.M. *of Virginia, &c. 8vo. Philadelphia. J. Dobson.*

It must be very evident that the anatomy, physiology and pathology of the nervous system, are subjects of far too extensive a nature, to allow of either of them being treated, with any degree of minuteness, within the narrow limits to which an inaugural dissertation is necessarily restricted. In the essay before us, the author has, therefore, attempted nothing more than a brief, but accurate outline of some of the more important points connected with each : in doing which, he has fulfilled, in a very happy manner, all that could be demanded of him in a thesis.

Though the production of a student, and laying claims to no higher character than that of a mere digest of the opinions and labours of others, we are nevertheless pleased that the essay has been committed to the press ; not only from the beneficial influence this will be found to exert over the future pursuits of the author, but, also, from the persuasion that its pages may be read with profit, by many who are engaged in the active duties of the profession, and who, being excluded from extensive libraries, and remote from the centre of medical intelligence, must be indebted to summaries like the present for their knowledge of the improvements daily making in the various branches of our science.

Such as we have described it, but little of novelty can be expected in the present publication. The curious reader will, however, be pleased with the following extracts—which may be adduced in confirmation of the opinion of the Royal Philosopher, that, " there is no new thing under the sun."

Speaking of the supposed modern demonstration of the origin of the nerves, Dr DUNBAR observes, " BARTHOLINUS, whom I shall quote more particularly hereafter, says expressly, 'the use of the lengthened and spinal marrow is to be the original of all the nerves; for from that part thereof within the scull, those nerves arise which are commonly attributed to the brain, being usually reckoned to be seven pair; but from the longest part thereof, which is [in] the back bone, anatomists do reckon thirty pair of nerves to arise, viz. as many as there are holes in the vertebræ.' " Again, the optic nerve, remarks Dr D. " is generally described by anatomists as originating from the thalamus nervi optici; but the late observations of GALL and SPURZHEIM, confirmed by TIEDEMANN and SERRES, show it to arise on each side from the tubercula quadrigemina." " The originality of this assertion has been supposed to be due to Drs GALL and SPURZHEIM; but the following passage from BARTHOLINE proves that the fact was not unknown to him. Speaking of the origin of these nerves, the latter says 'they arise not, as the common opinion is, from the forepart of the basis of the brain, for their original must be sought further towards the hinder part of the head, where they are carried between the brain and the beginning of the spinal marrow, and *arise out of the beginning of the first trunks* of the medulla oblongata, growing out of the brain. But RIOLANUS demonstrates that they are turned round about those eminences of the brain which GALEN calls thalamos nervorum opticorum, which reach into the foremore ventricles to fetch optic spirits from thence.' In a previous passage he styled the nates and testes the roots of the medulla oblongata."

" TIEDEMANN," remarks our author, " has clearly demonstrated that the spinal marrow is not an appendage to the brain, but that this latter is a production, or to use the language of REIL, an efflorescence from it; and he observes, that although this opinion was previously advocated by the ancients, yet to Dr GALL must be awarded the merit of having clearly proved it in modern times. But I am inclined to think that this gentleman has not such valid claims to originality in this respect. I find in BARTHOLINE the following language: ' We make the marrow, both as it is in the skull and in the back bone, to be the beginning rather of the brain; and that the brain being divided into two parts is, as it were, a certain double process or production of the marrow itself.' This he gives as a new opinion of his own: he then goes on with his proof; ' which is more manifest to those that behold the anatomy of fishes, for there the head and tail of the marrow is very great, but the process of the marrow or the brain is very little; the cause whereof is that fishes use motion more than sense; intimating that the brain or barke contributes more to sense, and the marrow itself to motion. Hence fish are dull of sense but very nimble in motion.' And again, a little farther on, he says, ' The lengthened marrow arises as some conceive from the brain alone, according to others from the brainlet or cerebellum. But it hath both (to speak now at a vulgar rate) for its beginning. For it arises from four roots or foundations, two of which are greater, from the fore part of the brain, commonly so called; two are lesser, from the inner part of the brainlet or petty brain. From these united the spinal marrow seems to be constituted. *But it is peradventure a more true opinion to think that these originals are processes of the marrow itself*, as was said before.'

" His language, also, with regard to the manners of dissecting the brain is, I think, entitled to a place here, as it is strictly applicable to the divided opinions and practice of the anatomists of the present day. 'Some with **Galen**, **Vesalius**, and **Fallopius**, intending to contemplate what is contained in the brain, begin their dissection in the upper part and proceed to the lower, and therefore do unfitly propound and explain many parts. I, treading in the steps of **Constantinus Varolius**, shall take quite a contrary course, yet such as is *true* and *accurate*, beginning at the lower part of the brain and so passing to the uppermost; and I shall afterwards propound the order of parts from top to bottom, for their sakes that will needs follow the *vulgar and common way of dissection*.'"

Dr D. also points out a very remarkable coincidence between the opinions of **Bartholinus** as to the formation of the lateral ventricles, and the views of the German and French anatomists*.

We have only room to notice the following fact, to which, although it was before not unknown to us, we feel pleased that the author has adverted. "Dr **Physick**, in his inaugural thesis, *De Apoplexia*, printed at Edinburgh in 1792, intimates very clearly his doubt of apoplexy being a result of gradual compression of the brain; and he consequently thinks that distention of the blood vessels or extravasated serum can hardly be received as causes." This is precisely the opinion of **Serres**, as maintained in his work on apoplexy, published a few years since. φ. x.

Article XVII.—*Mémoires sur le Traitement des Anus Artificiels, des Plaies des Intestins, et des Plaies Penetrantes de Poitrine. Par* J.-F. Reybard de Coisiat, D.M. *Ancien Chirurgien des Hôpitaux de Lyons, &c. A Paris,* 1827. *Pp.* 205.

Memoirs on the Treatment of Artificial Anus, Wounds of the Intestines, and Penetrating Wounds of the Thorax. By J.-F. Reybard de Coisiat, M.D.

This memoir possesses interest, not only from its intrinsic value, but also on account of its relation to Dr **Physick**'s method of treating the same distressing injury. For the opportunity of presenting an account of it, we are indebted to the politeness of professor **Gibson**, who liberally communicated the pamphlet to one of our number.

In an advertisement of fifteen pages, Dr **Reybard** gives an historical sketch of what has been hitherto done and attempted for the cure of artificial anus. M. **De la Peyronie** appears to have been the first among modern surgeons who recommended the attempt to be made to effect a radical cure: this he proposed to bring about by diet and compression. M. **Louis** refuted his theory; and showed that the real security for a cure depended on the size of the existing intestinal tube. **Desault** applied pressure to the dividing edge, between the upper and lower ends of the interrupted intestine; while he endeavoured to restore the lower one to its natural size by dilating it with plugs of lint.

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* See "**Bartholinus**'s Anatomy," published by N. Culpepper, Gent. and **Abadiah Cole**, Doctor of Physic. London, 1668.

Le Cat and **Bruns** proposed to cut off the edges of the unnatural orifice, and close them by the first intention; and caustic has been tried in the same way. All these methods were followed by serious consequences; and the number of cures obtained was very small. Next in order follow the proposal of **Smakalden**, and the operations of Dr **Physick** and M. **Dupuytren**; which we have noticed in Vol. II. pp. 269, 273 of this Journal. Mr **Collier**, an English Surgeon, has, since then, succeeded in radically curing an artificial anus by the Taliacotian operation. The edges of the anus were pared to the quick, and a flap dissected from the adjacent portion of the skin of the abdomen, and secured by stitches; which very curious proceeding met with complete success. M. **Jobert** has noticed the extreme difficulty of engrafting one portiôn of intestine within the inferior portion; in consequence of the want of disposition in the mucous membrane to adhere. M. **Denans** has succeeded in procuring adhesions between divided portions of intestine, in two dogs, by the following process. Two flat rings, of silver or tin, were thrust into the two ends of the intestine, in such a manner as to invert a small portion inwards, leaving the extremities covered by a fold of the serous coat. These were then brought in contact, and pressed against each other, by a third ring, which was introduced into them both, and secured by a spring. The whole was then returned into the cavity of the abdomen; the wound healed over then; the serous coats adhered; the portions turned inwards sloughed and fell off, and the rings were discharged by stool. This appears never to have been repeated in the human subject. M. **Colombe**, on the other hand, despairing of the radical cure, has invented an ingenious tube, to be attached to the artificial anus externally; so as to allow the fæces to follow their natural route, without any surgical operation.

M. **Reybard** considers the instrument which he has himself invented as an improvement, both upon the enterotome of M. **Dupuytren**, and the simple ligature of Dr **Physick**. He employs a double forceps, of light construction, one of whose branches is to be introduced into each of the two folds of intestine; while a slider, with a cutting edge, is to be protruded between them, in such a manner as to divide the included portions of the bowel; thus producing an incision of the same kind with that made by Dr **Physick**. By this process he hopes to create the lateral opening between the two intestinal tubes, without occasioning those violent symptoms which followed the operation of M. **Dupuytren**, and from which that of Dr **Physick** is not wholly free. The substitution of cutting, for the severe process of artificial mortification produced by the former surgeon, must certainly save the patient from great sufferings and dangers, if, at the same time, he be equally free from the appalling risk of peritoneal inflammation. Whether the surfaces retained in contact by the instrument of M. R. will adhere with sufficient certainty to produce a vital closure of the cavity of the abdomen, and thus prevent such alarming consequences; or whether it will render the probability of the latter less, than in a case where severe inflammation, and gangrene are produced in an intestine, are questions which can only be settled by experience.

The instrument of M. Reybard is composed of a *body*, two *branches*, and a cutting *blade*. The body is nearly similar to a spring-forceps such as is used for dissection, but cut off at the length of an inch and a half. From *each* of these terminations runs a *branch*, composed of *two* stalks of steel, united at their ends by a rounded termination, and leaving a space of about two lines between their parallel sides. When the instrument is closed, the spaces thus included in the two "branches" correspond with each other; so that the cutting blade can move through both at once. The branches are four inches long; and so slender and elastic that, with whatever force the forceps-shaped part is closed, it is impossible for them to bruise the intestines, but the latter will merely be kept in contact with each other. In using the instrument the points must come in contact first; and the fissures in the branches, destined to receive the cutting blade, must be a little wider at their commencement, with a large previous space for the introduction of the blade. The latter is affixed to the two sides or stalks of the upper branch by two projections like the heads of screws, divided lengthwise to receive the steel stalks; and the whole instrument must be fitted to perform its motions with neatness and certainty. The compression of the two branches or blades of the forceps is made by two screws, one on each side.

The cutting blade is a long lancet, or instrument like the blade of Dr Physick's gorget, provided with the two projections already mentioned, which must be capable of turning round. The external part of the edges of the cutting blade must be blunt, and a little thickened, to protect the adjacent parts; and the blade should be prevented, by the construction of the instrument, from approaching, within a considerable distance, the extremity of the branches. This is not only to prevent the edges of the blade from being blunted, but to obviate the danger which occurs when the instrument is slightly retracted by any accident; an event very liable to occur; in which case, effusion may take place into the cavity of the abdomen, and produce the most fatal consequences.

The first case in which M. Reybard applied his instrument was that of a woman of Dauphiny, aged 36 years, who had been affected for a month with a large wound or ulcer in the lower part of the belly and in the right groin, through which she discharged all her fæces. The origin of this injury is not given; but as the intestine emerged from beneath the crural arch, it is presumable that the case was one of femoral hernia. The ulcer was very large and deep, with abrupt, red, and inflamed edges, and the surrounding parts were also red to a considerable distance. In the bottom of the wound, the lower portion of intestine formed a small tumour, so prominent that M. R. found it necessary afterwards to extirpate a portion of it in order to bring it within the level of the skin, for cicatrization. The end of the upper portion, through which the fæces escaped, was so deeply immersed in the wound that it was necessary to distend the skin with considerable force in order to bring the orifice into view.

Into the orifices thus situated, however, M. R. was able to introduce, with facility, the female catheter; and, as this was the case, he judged it unnecessary to widen the wound, but immediately proceeded to insert the two blades or branches of his enterotome. Standing on the right side of his patient, he experienced no difficulty in doing this. Each branch was placed in its corresponding intestine; the screws were tightened, and the cutting blade attached to the superior branch of the instrument on which it was made to slide. Holding the forceps in the left hand, the operator then pushed the cutting blade along its guiding branch; and thus effected the division of the two intermediate portions of intestine, without the least difficulty, and with so little disturbance to the patient that she continued "knitting" during the operation, of the serious nature of which her surgeon had not made her aware. When M. Reybard, previously to making the section of the membranous partition, had *twisted* the instrument, with the view of gaining an additional certainty of its two parts being correctly placed, the strain thus inflicted on the bowel produced a painful sensation of *dragging* in the parts within, but the section appeared to be altogether without pain. No means are mentioned by M. R. as having been employed to ascertain that no other fold of intestine, than that designed, was included in the grasp of the enterotome. From his description, we presume that he depended, for this, upon the close proximity and parallelism of the branches, and their meeting with no resistance during their simultaneous introduction. For two days, the patient continued to lie on her back, but experienced no inconvenience except from a voluntary retention of urine; a restraint to which she had subjected herself from the want of instructions, and which had gone near persuading M. Reybard of the existence of *peritonitis*—a very fair specimen of the anxieties of an operation in new circumstances, the patient being affected with restlessness, heightened colour, and abdominal swelling. On the second morning, the usual fæcal discharges were found on the dressings; but after this, nothing was there met with but a little yellowish liquid. In 47 hours, M. R. withdrew the forceps: he then found that his little finger passed with ease into the orifice, and entered the abdomen under Poupart's ligament, but he could not reach the undivided portion of the membranous partition. He judged that the incision passed an inch within the ligament: and had it not been sensibly within the latter there must have continued an escape of fæces. To this a stop was effectually put; and an immediate improvement was visible both in the condition of the wound, and in the patient's general health. The escape of fæces, however, returned when the patient was allowed, ten days afterwards, to leave her bed; and M. Reybard did not succeed in completely closing the small stercorary fistula which remained, until he confined the patient's thigh in close approximation to her abdomen, by means of bandages; thus favouring the cicatrization of the wound by position. In 25 days, however, this took place, and the woman was discharged; requiring no other treatment than a hernial bandage and compress, such as are substituted for a truss.

His next case of artificial anus was in a boy 7 years old, who had received a penetrating wound of the belly. He was affected with a tumour of considerable size, formed of a portion of the great arch of the colon, inverted on itself; and situated on the left side of the scrobiculus cordis, under the edge formed by the cartilages of the ribs, and three fingers' breadths below the extremity of the xiphoid cartilage. The lower end of this tumour was divided into two portions; of which that on the right hand eventually appeared to be the extremity of the lower portion of the divided intestine, covered both within and without by its own mucous membrane, which had been forced, in redundant folds, out of the intestinal cavity. The left end appeared to be one of the cells of the colon, enlarged, and crowned, at its most pendulous extremity, with copious vegetations. The anal opening, belonging to and terminating the cavity of the upper portion of intestine, was on the right side of the tumour, near its origin; and this was surmounted by a singular excrescence, larger than a hen's egg, appearing to be formed of protruded mucous membranes, and exhibiting two additional openings, which originally gave to the case the appearance of having two artificial anuses, but which eventually were found to have no connection with the intestinal cavity, though they communicated with one another. The formation of this singular structure M. Reybard endeavours to explain; but we frankly confess ourselves unable to understand his description. M. R. cut off the tumour containing the two additional orifices, leaving the pendulous tube, which it was his intention subsequently to use as a plug, to close the mouth of the wound. He also attempted to reduce the pendulous tube, but soon found this impossible from its hardness and adhesion. He next made an incision into it, of an inch and a half in length, opposite the anal orifice, but found considerable difficulty in completing this part of the operation, from the redundancy and laxity of the internal mucous membrane, which retired before his bistouri. After completing this incision, he examined the adjacent portions of the intestine still retained within the abdominal cavity, by means of his fingers and the female catheter. The intestines were found to meet each other at a right angle, instead of being placed, as in many other cases, parallel, like the two tubes of a double-barrelled gun. From this cause, considerably more force was required to bring them in contact and retain them, than that exerted by his enterotome in the preceding case. He consequently was gratified to recollect that he had provided a stronger one. This was introduced with some little delay, arising from the thickness and divergency of the included parts; but the instrument was finally screwed up, the cutting blade adjusted, and the section of the membranous partition completed, without any further difficulty.

The patient was then left in bed, placed on his back, with the forceps of the enterotome secured in its place by being attached to two turns of bandage fixed round his waist; and the wound was closed with lint, spread with simple cerate, and surmounted with compresses.

We omit part of the details of the treatment. The little patient was troublesome and ungovernable; and various articles were given him for food, which were considered highly improper. Nevertheless, with some retching and fever, he got through the first two days; and, at the end of forty-eight hours, the instrument was removed. The pendulous tumour was found in a state of lively inflammation, and did not return to its usual size for two weeks. Moderate pressure, however, by means of lint, compresses, and bandages, was found completely to control the progress of the fæces; and from this moment the child had one or two stools every day. As soon as the inflammation had entirely disappeared, M. R. raised the pendulous tumour, and gradually applied it to the wound; during which process, it greatly diminished in size, and completely filled up the orifice.

At the end of about a month, the child had a severe indigestion from excess in eating; which increased a diarrhœa under which he had been labouring, produced colic, fever, marked irritation throughout the whole course of the alimentary canal, sensibility of the abdomen to pressure, and the discharge of an immense number of lumbrici both by the natural anus and by the wound. The fever became very severe, *took the adynamic character* (typhus mitior), and the diarrhœa continued to increase. The little ungovernable sufferer would neither drink what was offered to him, nor submit to injections. Emollient applications, including embrocations with oil, were made to the belly; but, for medicine and ptisan, the surgeon was reduced to the employment of simple water. Under this treatment the fever diminished: but the diarrhœa and prostration of strength continued, augmented by the indigestible articles of food which the attendants thought themselves obliged to permit the child to eat; such as potatoes, chestnuts, and the rind of bacon. Two months were consumed in this manner; and the emaciation became so great that M. REYBARD sent him to the country; despairing of success in any farther operation until his health should be established. When this should be the case, of which M. R.'s last information encouraged him to entertain hopes, it was his intention to endeavour to engraft upon the orifice a piece of the inverted intestine; removing for this purpose the edges of the opening, and one of the mucous membranes of the intestine. The irreducible portion was then to be amputated, and the cavity of the part employed as a graft, destroyed with caustic potassa. Should this fail, a slip of the adjacent skin was to be employed in the same manner, by the Taliacotian method; a process which has already proved useful in his hands, in an instance to which we shall shortly allude.

This curious case is also remarkable for its origin. The primary injury had been a wound of the abdomen, without any puncture of the intestine; but the surgeon, instead of simply reducing the bowel, and closing the wound, thought proper to *affix a ligature quite round the intestine,* thus producing, gratuitously, all the phenomena of strangulated hernia. We have asked ourselves the question, how far this might have produced the two unusual openings which appeared to traverse merely a portion of the mucous membrane. Could not such an effect have ensued from the operation of a ligature, lax enough to permit a mass of the mucous membranes to pass through it, but capable of making them slough after they had become tumefied? This "singular operation," occurring in so scientific a country as France, affords us, we must confess, some comfort for the bungling occasionally seen in our own.

M. REYBARD next inserts the account of Dr PHYSICK's operation, as translated from the fourth number of this Journal. "This," he states, "may be considered as the starting point of all the improvements which have been made in the treatment of unnatural or anormal anuses."

The case which is placed first in the memoir of M. REYBARD is truly a most interesting and curious one; but it does not furnish an instance of the application of his enterotome. Our abridgment of it must, therefore, be extremely brief. A soldier received several wounds with a cutlass, through one of which M. REYBARD found all the man's intestines protruded, and part of them lying beneath the pressure of his back and hips. Two wounds of intestines were found, which M. R. retained externally, while he reduced the remainder. One of these was nearly at the commencement of the ileum, and consisted in a slit of an inch and a half in length, situated obliquely, with its edges greatly reverting themselves. M. REYBARD cut a flat piece of fir wood, 15 or 16 lines by 8 or 9, through which he passed the ends of a long ligature, so as to appear on the same side, about 3 lines apart. These ligatures were then armed with needles, and passed through the edges of the intestinal wound; and afterwards combined in the eye of a single needle, and passed from within outwards, through the abdominal parietes. M. R. then carefully adjusted the lips of the wound in such a manner as to place their peritoneal surfaces in contact with the lining membrane of the abdomen, drew the ligature moderately tight, and tied it over a little roll of linen.

In 48 hours the ligature was cut off, and, shortly after, the piece of wood was rejected through the artificial anus formed by the other wound.

This last was a puncture of the colon, which had been torn open, until it embraced half the circumference of the intestine. The remainder of the case is most remarkable for the use of a silver tube, with the object of confining the fæces, for the time, to their natural course, allowing the external wound to contract, free from the irritating contact of the fæces, and keeping the inferior orifice from excessive contraction; for the employment of the Taliacotian method, under very unfavourable circumstances, with great advantage; and for the achievement of the final cure by the *position* described in the case we have placed first in this analysis. In this case, as may well be supposed, severe peritoneal inflammation took place, and was combated with the utmost difficulty.

To this curious and valuable document are appended a course of experiments on dogs, a short memoir on penetrating wounds of the thorax, and a case of exostosis of the cranium; of all which we can only regret that our limits prevent us from presenting an abstract.

It was found, in a dog, that a *tight* ligature upon the flat piece of wood was liable to occasion gangrene in the peritoneum; and that if the ligature was applied over a hard substance, the integuments were apt to inflame in a troublesome manner. The slip of wood came away, without inconvenience, per anum; and on dissection, six weeks after the operation, the adhesion round the wound was found converted into a membranous bag, communicating with the intestine. Of six trials of these experiments, four succeeded, one suffered an artificial anus, and one died in four days, after suffering much by colic and vomiting. More than twenty attempts to engraft one intestine within the other, as advised by writers, uniformly failed; while, by means of the previously described enterotome, the lateral union was accomplished with ease in four cases. Dissection showed the edges of the divided double membrane, united together and projecting into the intestinal cavity, and only effaced where the intestines adhered to the wound. This latter was not closed, until after the destruction of the lining mucous membrane, by a tent, composed of lint, wet with a solution of caustic.

ARTICLE XVIII.—*On the Influence of Medicine. An Oration delivered before the Philadelphia Medical Society, pursuant to appointment.* By JOHN BELL, M.D. *one of the Corresponding Secretaries of the Society, Lecturer on the Institutes of Medicine and Medical Jurisprudence in the Philadelphia Medical Institute. Feb.* 9, 1828. *Published by the Society. Philadelphia,* 1828. *Pp.* 36.

So much difficulty is experienced by those who are in search of topics yet unexhausted, upon which to found the annual orations of societies, that we think DR BELL really happy in having been able to select one, which, if we have previously heard discussed, we have forgotten the circumstance. And, were it not so, there is so much usefulness in thus recalling to the minds of professional men the claims of their vocation to the possession of high moral dignity; it is so elevating and ennobling, that sentiments of this kind can hardly be too often repeated, or too strongly enforced by the facts derived from history.

The object of the oration before us, is to exhibit, by a series of undeniable truths, the vast influence in favour of the moral and intellectual welfare of society, which has been, at all periods, exerted by the profession of medicine. The art of healing was, in the heroic ages, the attribute of a demigod; in Egypt and the Orient, that of the ministers of religion. And when in later times, wise and learned Greece substituted reason and exactness for the idolatry of the imagination; when truth and argument began to replace the decayed fires of poetical enthusiasm, she ranked her first literary physicians among her philosophers. From that hour to the present, amid all the vicissitudes of weal or woe which have befallen the human race; whether learning degenerated within the pompous but servile walls of decayed Byzantium; slept in the convent, to be occasionally awakened by some solitary monk, illumined beyond his age, but forgotten in its impenetrable oblivion; or flourished, amid the barbaric magnificence of Moslem greatness, in Bagdad, or in the Alhambra; still, through all these changes, we universally find the medical character united to philosophy and the love of doing good. The profession is essentially *philosophical;* and this is not confined to the study of abstract truth, but extends to the ethical duties of daily life. In no other is more knowledge of men required, for the discharge of the most essential and imperative obligations; nor are the medical rules of hygieine any other than an exemplification of those principles recommended by the wise of every age for the due and reasonable enjoyment of happiness. Neither does the philosophy of our profession end here. It is also necessary for the physician to set the most rigid bounds to his own love of wealth and reputation; to love his profession in the abstract, as a means of doing good, and as a noble employment of the faculties with which Providence has endowed his species. The constant exercise of charities, unrewarded either by fame, future patronage, pecuniary recompense, or private gratitude, is, we boldly assert it, in no class in life carried to so great an extent as in our own: and, indeed, without this sense of usefulness, and of the very partial value of riches and fame, it would be impossible for the greater number of physicians to abide the situation in which they are placed.

Of these elevated views we have had numerous bright examples; many of which are collected by our author, from the histories and biographies on our book shelves. Yet it is by no means necessary to refer to printed records for illustrations of this truth. Every medical man can probably recall, within the reach of his own memory, instances of the persevering practice of all these virtues, in the person of some quiet, unpretending man of labour, whose life has been spent in ill-rewarded toils, without his ever aspiring or being admitted, to the pages of historic commemoration. The Christian's creed teaches that he will have another reward; and it is necessary that he should, for he does not obtain an adequate one from his fellow men.

Dr BELL's oration is learned as well as eloquent; and if we miss, in his enumeration, the names of SYDENHAM and BICHAT, it is probably because he considered their influence upon the public as too indirect, and as having been exercised mainly through the medium of their brothers of the profession. The former was endeared to his fellow citizens by defying, on their behalf, the terrors of "the pestilence that wasteth at noon-day;" and the latter, by his genius, accuracy, and love of truth, was able to achieve the renovation of the French medical schools, and to elevate the character of his profession in the eyes of the public, by transferring to realities the exercise of those talents which had been so long wasted in authority and conjecture. But why should we seek for further instances, or endeavour to enlarge the list already furnished by our author of shining examples of what a physician may and ought to be? Enough has been done, within the limits of a brief address, to demonstrate the fact mainly in view; and more would not be sufficient to overpower the illiberality of those who would liken a profession like ours to any of the ordinary honest but selfish modes, by which men secure to themselves pecuniary wealth and importance in the community.

Article XIX.—*American Medical Biography; or Memoirs of Eminent Physicians who have flourished in America. To which is prefixed a succinct History of Medical Science in the United States, from the first settlement of the country. By* James Thacher, M.D. *Fellow of the American Academy of Arts and Sciences; Honorary Member of the New York Historical Society, and of the New York Horticultural Society, &c.: Author of the American New Dispensatory, of the Modern Practice of Physic, and of the Military Journal. Two volumes in one. 8vo. Boston,* 1828.

The venerable author of the above mentioned work deserves the thanks of the public for this successful attempt to rescue from oblivion the names and histories of many excellent physicians who have honoured the medical profession in America.

We shall not attempt in this small space to describe his work further than by saying that it contains 159 biographical articles, some of which are written at great length, and with much elegance. Every physician who respects himself, is interested in the preservation of such materials as these; for every such man cannot fail to desire at least, that after the world and its temptations have tried him, he may not be found wanting in those qualities which entitle him to a place in such a collection.

Many a bosom has been warmed with emulation of those virtues described in the biographies of Plutarch; and it is a happy tendency of just and proper writings of this sort, to awaken and cherish the seeds of virtue, that might, without such culture, have lain dormant for years.

May the venerable author reap, for many a year, the harvest of reputation which here, and in other fields, he has planted.

ARTICLE XX.—*Constitution of the Medical Faculty of Caracas. Pamphlet. Caracas*, 1827. *4to.*

We have received from our friend Dr SAMUEL BROWN, a copy of the constitution and laws of the Medical Faculty of Caracas, established in 1827, and for which he is indebted to Dr Vargas, of that city.

This body, which is authorised and even established by governmental enactments, has received the salutary power of examining and deciding on the claims of all persons who propose to practise physic in that country. It is also permitted to exercise a wholesome surveillance over the morals of practitioners, the conduct of apothecaries and druggists, and, in general, over all the interests which are concerned in elevating the character of the medical profession. These powers, if cautiously and wisely administered, will doubtless go far in redeeming medicine from the disgrace which ignorant pretenders are constantly bringing upon it in all parts of the globe. Thus science flourishes when protected by liberty, to whose temple she is led by the supporting hand of the *Liberator*.

We blush to say that in some of our comparatively old states no legislative provision, like that of Caracas, has been made for the security of those great professional interests, which sooner or later, by accidents or disease, are brought home to the business and bosoms of all civilized men.

Let us hope that a frequent and friendly intercourse of the physicians of both countries may give a new impulse to our noble profession. Let us hope that this excellent example from the south may be followed here in the north, and that in exchange for our political models, they may render back to us a more excellent pattern of zeal and devotion for the advancement of knowledge. In knowledge alone, rests the basis of real power and perpetuity for the institutions of both countries.

Quarterly Summary

OF

Medical and Surgical Intelligence.

I. ANATOMY.

1. M. J. **Arthaud** *on the Formation of the Cellular Tissue.*—The question M. **Arthaud** proposes to himself is, whether the cellular tissue is really composed of lamellæ and filaments, which, when united, constitute areolæ of a determinate form, or whether the areolæ are merely accidental, being produced by the dissector in handling it. **Malpighi**, **Ruysch**, **Haller**, **Bichat**, **Beclard**, and many others are of the former, while **Bordeu**, **Blumenbach**, **Meckel**, **Autenrieth**, **Prochaska**, &c. &c. are of the latter opinion. **Meckel** prefers to call it mucous tissue, and says it is coagulated; that when we move or pull it, it takes the form of filaments and lamellæ; that we can shew its cells by blowing air into it on the same principle that we can shew the cells in soap suds, or any mucus if we blow air into it.

2. *Stomach ending in a Cul de Sac.*—M. **Crooks** in a German journal relates the case of a child which in external appearance was perfect. Six hours after birth it began to vomit a brown liquid. The vomiting soon became bloody, and when sixty-five hours old it died. The pylorus communicated with a sort of second stomach, or bag, which had no other opening except that of the pancreatic duct. It had no communication with any intestine.—*Journal des Progrès.*

3. **Biancini** *on the Communication between the Blood-Vessels of the Uterus and Placenta.*—M. **Biancini** injected the blood-vessels of a woman who died in child-bed with inertia of the womb, the placenta being still undetached from the uterus. He found the injection in the vessels of the chorion and amnios; and having examined the tortuous arteries of the uterus, he found that they penetrated the tissue of the placenta, spread themselves over its membranes, and deposited the injection in the cells described by **Hunter** and **Meckel**. In a young woman who died eight days after delivery, a small portion of placenta retained its adherence to the womb. In this case, injection forced into the uterine arteries not only passed into the residuary portion of placenta, but also was extravasated into the cavity of the womb, and into the vagina, from the lacerated ends of what he calls utero-placental arteries.

But the most extraordinary facts in support of M. **Biancini**'s opinions are those elicited from the examination of the subject in brutes. Not only did he find in cats that the colouring matter of fine injection passed to the umbilical vein, but he actually found the vermilion to have tinged the vena cava superior and some of its smallest ramifications. Such at least is the statement in the Journal des Progrès. We see not very readily why the vermilion should be found in the *upper cava*.

M. B. to make assurance doubly sure, next inverted the modus operandi. He injected mercury into the umbilical arteries of a calf, and found not only that it passed into the placenta, but directly into the uterine veins by nine short cylindrical branches, which he calls placento-uterine veins.

II. PHYSIOLOGY.

4. *Dr* MERRIMAN *on the Duration of Pregnancy.*—The question of the duration of pregnancy has never been satisfactorily settled, and as it is an important point in medical jurisprudence, the acquirement of some certain knowlege in regard to it, is extremely desirable. Dr MERRIMAN constructed the following table, which he laid before the Medico-Chir. Society of London, and which is published in Vol. XIII. Part II. of their Transactions. Whenever the Doctor has been requested to calculate the time at which the accession of labour might be expected, he has reckoned forty weeks from the last day on which any appearance of the catamenia was distinguishable, assuming that the 280th was to be considered as the legitimate day of parturition:

"*A Table of the Births of* 114 *mature Children, calculated from, but not including, the day on which the Catamenia were last distinguishable.*

	Births.
At 255 days	1
256 days	1
259 days	1
In 37th week	3
At 262 days	2
263 days	2
264 days	4
265 days	1
266 days	4
In 38th week	13
At 267 days	1
268 days	1
269 days	4
270 days	1
271 days	2
272 days	2
273 days	3
In 39th week	14
At 274 days	4
275 days	2
276 days	4
277 days	8
278 days	3
279 days	3
280 days	9
In 40th week	33

At 281 days	5
282 days	2
283 days	6
284 days	1
285 days	4
286 days	3
287 days	1
In 41st week	22
At 288 days	5
289 days	2
290 days	2
292 days	4
293 days	2
In 42d week	15
At 295 days	1
296 days	2
297 days	2
298 days	4
301 days	1
In 43d week	10
At 303 days	1
305 days	1
306 days	2
In 44th week	4 ”

Dr M. thinks that we may infer from the above, that conception more frequently occurs soon after menstruation, than immediately before the recurrence of that discharge.

In a note is the case of a girl who swore to having had only one connexion, viz. on the 8th Jan. by which she conceived, and her child was born on the 18th October, which is 283 days.

5. *New Views of the Fœtal Circulation.*—We find in the Bulletin des Sciences Médicales for February, a notice of the work of Dr H. Fr. Kilian, published at Carlsruhe in 1826.

In the work is contained an account of the development of the heart of the fœtus, from its first appearance until the third and fourth months of gestation. M. K. asserts that in the early stages of pregnancy the inferior cava opens both into the right and left auricles by distinct mouths. At the commencement, the *left* opening is much the largest; but towards the close of the 9th month, the *right* has become much the largest. The valve of Eustachius and the valve of the *foramen ovale* constitute part of the inferior cava, and are to be considered as duplicatures or prolongations of the internal coat of the vein. These valves appear to be formed at the same epoch; but the valve of Eustachius is more rapidly and perfectly developed than the other. The valve of Eustachius, which principally constitutes the orifice of the inferior cava of the right auricle, serves to direct the two great columns of blood brought by the superior and inferior venæ cavæ, so that one may pass before the other without interference or mixture. The valve of the foramen ovale, or rather the left half of the lower cava is a *vascular branch*: its use is gradually to effect the exclusion of the *cava inferior* from the left auricle, and favour its passage or transfer to the right auricle. The septum of the heart is not perforated; the foramen ovale passes behind it through a notch. There is, properly speaking, no pulmonary artery in the fœtus. The trunk that bears the name is continuous with the *canalis arteriosus*, and constitutes the origin of a vessel which goes to the lower half of the body, and may be named *abdominal aorta*, to distinguish it from the cerebral aorta which rises from the left ventricle ; so that the fœtus has a double arterial circulation, one derived from the right ventricle, which goes to the body, and the other from the left, which supplies the head.

M. Kilian's doctrine of the fœtal circulation is as follows. Blood comes from the cells of the placenta through the umbilical vein to the liver; it is there divided into two unequal portions, the largest of which circulates in the vessels of the liver, while the smaller portion goes through the *ductus venosus* into the vena cava. Here the blood of the ductus venosus, of the liver, and of the lower extremities is mixed. Just before it enters the heart, the current is divided, as above stated ; one stream enters the right at the same moment that the other enters the left auricle. Upon the contraction of the auricles the ventricles are filled, and in their turn, they throw it out *without any mixture taking place ;* for that of the right goes to the descending aorta exclusively, and that of the left is distributed upwards. The author asserts that if different coloured injections be thrown into the aorta and pulmonary arteries of a fœtus at the same moment, no mixture of colours will take place. The rest of the circulation, namely back into the veins, he explains in the ordinary manner.

6. *Anomalous Dentition.*—In the *Archives Generales* for Jan. we find the following, cited from the *Diario de las Ciencias Medicas* of Barcelona. Morelli, a tall well formed woman, had always enjoyed good health ; she had suffered some attacks of toothach, which were easily relieved by the loss of blood. She was the mother of four children, and was again pregnant in March 1821, when she was afflicted with pain in the two molar teeth of the left side. The teeth were drawn, and she was relieved. In the following October the pain reappeared, and was followed by the eruption of two sound and perfect molar teeth in place of those she had lost. These teeth were always loose and troublesome, so that she had them extracted ; but the pain after a considerable time returned, and two more teeth were cut in the place of them.

7. *Average Duration of Life.*—"M. Odier has been lately engaged in investigations on the average duration of life in Geneva, all of which show a gradual extension of the period. The inquiries of M. Odier go as far back as 1560, and may be fully relied on. From 1560 to 1600, the average duration of life was eighteen years and five months; from 1600 to 1700, it was twenty three years and four months ; from this time, the period became gradually longer ; and, from 1815 to 1826, the average was thirty eight years and ten months."—*Lancet.*

8. *Congenital Absence of Sensation and Voluntary Motion in a Female.*—This girl was born rather prematurely. She gave no sign of the usual instinctive desire for the breast ; was, with difficulty, taught to suck, and appeared to be very feeble. One eye was smaller than the other. A few weeks after birth she was seized with convulsions : no medical prescriptions had any power to restrain them, and they exceeded a hundred in twenty-four hours. As she continued to grow in length, the defects in motion and sensation became more apparent. Blindness came on, she became quite deaf, and never had a second dentition. Taste seemed tolerably perfect. She never acquired any thing like the power of voluntary motion, and attained her 17th year without having held up her head, raised her hand to her mouth, or set her foot to the ground. She never uttered any thing like an articulate sound, and a feeble cry or whine was believed to indicate the want of food. When fed with sweet articles, the countenance exhibited an expression of satisfaction.

She never had a stool without medicine, and never acquired any command over the sphincters. On the day when she completed her 17th year, she died in a manner so gradual that the attendants thought her asleep, when she was really dead.

With regard to the morbid appearances on dissection, the following alone seem to possess any interest.

On removing the brain from the basis of the scull, so as to expose the nerves, he could trace each pair taking their usual course to their destination; and they all appeared firm and healthy, as in the most intelligent individual. But in the base of the scull itself not a vestige of the dura mater was to be seen. Its place was supplied by a thin semitransparent membrane, very lax and irregular, so that it afforded no protection to the nerves in their exit from the scull.

On the back part likewise, the whole or the greater part of the *tentorium* was deficient, thus allowing the whole weight of the brain to rest on the cerebellum.

The author's opinion is that the girl's deficiencies were occasioned by the want of dura mater on the base of the scull, the mass of brain being thereby allowed to press on the nerves of sense as they passed through their separate foramina, and thus their vital principle became destroyed; and that the cerebellum, not protected by a tentorium, was also pressed upon by the cerebrum, which also suffered in its turn.

III. PATHOLOGY.

9. *Dr Bayle on Insanity from Chronic Gastro-Enteritis.*—From the cases presented in our last, and from others of a similar kind, Dr BAYLE draws the following inferences:

1st. Chronic gastro-enteritis is capable, in a few instances, of occasioning mental alienation; and in very many cases, can prolong its continuance, modify it, and determine its character.

2d. Most of the insane who become so from a chronic gastritis, or gastro-enteritis, derive from their parents, or from their own constitutions, a strong tendency to delirium; their faculties are generally weak, and their brain prompt in taking on irritation. Frequently, also, the fears and anxiety which they suffer from the existence of the original disease, predispose them to insanity.

3d. The *anatomical* characters of chronic gastritis, or gastro-enteritis, when productive of mental derangement, do not by any means differ from those which are found where the same diseases exist without the subsequent occurrence of insanity.

4th. The organic changes going on in the intestines act upon the head in two ways. The first is by direct sympathy, precisely as the heart is affected at the same time, so as to produce fever. This occurs in the more violent cases, and produces mania, with a total derangement of the understanding. The other mode of operating on the head is by the pain and uneasiness undergone in the abdomen, which thus produce melancholy, hypochondriasis, and monomania. These agencies have been already enumerated as predisposing causes. See Proposition 2.

5th. The *symptoms* of sympathetic mania are referrible to mania and melancholia. Dementia may succeed to both these forms if they be of long continuance. The premonitory symptoms are the same in both instances; being indicative of disorder in the abdomen. Sometimes these are accompanied by a train of nervous phenomena extremely diversified in their character, and which, when combined with the fears of the patient, constitute a real hypochondriasis. Between the symptoms of actual mania thus produced, and those of the common kind, there exist no very essential distinctions. Yet, in the midst of the most furious excitement, we very frequently hear the most lively expressions of the fear of being poisoned; and often find the sufferer obstinately refusing all food—phenomena which Dr BAYLE considers as being produced by the abdominal pain, not as the subject of distinct conception and a rationale, but as giving a character to the delirium.

M. B. regards the *fear of poison* and the *refusal of food* as the most essential and invariable symptoms of mental alienation, when accompanied by chronic phlegmasia of the digestive mucous membranes. They are owing, as has been already said, to the pain occasioned by the inflammation; and make their appearance alike, whatever be the condition of the intellectual faculties and the form of the delirium. We observe them both in melancholy, mania, and dementia; but principally in the first species, whether this be the product of the sympathetic irritation of the brain, or whether, arising from mental causes, it become afterwards complicated with gastro-intestinal irritation. These two symptoms are a real expression of the suffering of the stomach and intestines in the deranged; and, although they may exist without owing their origin to this cause, M. B. has never found them without concomitant signs of inflammation of the stomach. This remark is so well founded, that they sometimes exhibit themselves in a transitory manner in those insane who are temporarily attacked with acute gastritis. They are even seen in some whose brain is not deranged in its operations. M. B. has seen three patients labouring under chronic meningitis, with general and incomplete palsy, who were affected, during 7 or 8 days, with unusual redness of the tongue, and an increased frequency of pulse; and in these there likewise occurred a fear of poison and a refusal of aliments; both which peculiarities disappeared along with the gastritic symptoms already enumerated.

The local symptoms, which it would be very desirable to ascertain with correctness in this disease, are difficult to observe. The motions of the patient prevent the physician from ascertaining the condition of the tongue, the pulse, and the epigastrium. When the pulse has been felt, febrile excitement has often been discovered; a thing very naturally to be expected, but which is not invariably present. Diarrhœa is very common, and often fluctuates in a manner coinciding, in a remarkable degree, with the condition of the mental faculties.

The progress of the sympathetic mania may be either continued, remittent, or intermittent; but the first mentioned type is much more frequent than the others. When this is the case, the agitation and fury continue for a longer or shorter time, without offering any sensible diminution. Their duration is variable, like that of common insanity.

This malady may terminate in various ways. Sometimes the patients suddenly become calm and reasonable. The return of reason being accompanied by that of their ordinary powers of sensation, they complain of pains in the epigastrium or other parts of the abdomen, of headach, and of general uneasiness. Sometimes the tongue is red and the pulse frequent; and diarrhœa is a common symptom. In some instances, the gastric symptoms produce hypochondriasis, and furnish a serious impediment to the patient's ultimate recovery of the strength of his intellectual faculties.

In another set of cases, the agitation ceases suddenly, while the delirium persists, and the patient soon sinks; the physician seeing no other visible cause for his demise than the violent agitation to which he had been subjected. Commonly the tongue, previously to death, is found red and dry, and the pulse frequent; and diarrhœa often exists. Sometimes the maniacal fury gives way to a general collapse, in which are exhibited all the appearances of a *typhoid* gastro-enteritis. The lips are dry and brownish; the teeth and gums fuliginous; the tongue, red, dry, and brownish; there is a copious diarrhœa; the pulse is small and frequent; the decubitus a profound supination, and the faculties in a state of coma. Death soon terminates this catalogue of distresses. In other patients, there takes place a gradual marasmus, in consequence of the diarrhœa; which commences early, and slowly exhausts the sufferer: here his spark of life goes out with but little struggle, but without any of those consolatory intervals of reason and quiet, which sometimes meet the eye of the physician, but which are magnified in their frequency by the enthusiasm of the poet and novelist.

Finally, as already mentioned, a state of dementia may succeed to sympathetic mania; and may exist for various periods.

6th. The *treatment* recommended by our author for insanity complicated with gastro-enteritis, is to be directed in an especial manner against the abdominal phlegmasia. The remedies to be used are those which are adapted to the cure of the last mentioned malady, when not accompanied by cerebral symptoms; and are to consist chiefly in the application of leeches to the epigastrium, in mucilaginous drinks, in a diet composed of aliments light and of very easy digestion, exercise, and sometimes derivatives. In addition to this, the mental derangement will require, according to the form which it assumes, the same means which are useful in cases of idiopathic mania: "such as seclusion, strong moral impressions, the shower-bath, reasoning with the patient, the strait-waistcoat, &c."

These observations of M. BAYLE may be considered as affording the means of distinguishing with greater precision those cases, in which the diseases of the mind are owing to irritation in the abdominal cavity—a curious subject of inquiry which has employed many minds. The opinions of the ancients respecting *atrabilis*, as a source of melancholy, and their treatment of insanity by purging with hellebore, must have been founded on observation; and, among the moderns, DUFOUR, PINEL, PROST, and ESQUIROL, have collected a mass of facts and arguments well calculated to enforce the idea.

10. *Small Pox analyzed physiologically.*—We continue, in this and one or two of the following articles, our series of cases extracted with a view to illustrate the history of gastro-enteritis. M. LEFE'VE'REND, of Havannah, communicates two cases of the above-mentioned disease to the *Annales* of professor BROUSSAIS. We meet, prefixed to them, the 142d proposition or aphorism of the latter pathologist, which we translate.

"The small pox commences by an acute gastro-enteritis, which is the first effect of the contagious cause. The phlegmasia of the skin replaces it and terminates it when the pustules are few in number; but, if the pustules are numerous, they regenerate it, by the erysipelas which results from the confluence of the areolæ. This forms the secondary fever of the small pox, which is also called the fever of suppuration."

According to Dr L., the small pox reigned as an epidemic at the time of his arrival at Havannah; and the bad treatment there opposed to it rendered its mortality frightful. His first patient was a negro, who was seized, in the morning, while working at the wharf, with retchings to vomit, and with pains in the limbs and in the forehead, just above the orbits of the eyes. Dr L. was called in the evening, and found him with a supine decubitus, coma, his tongue red at the edges and point, and coated with mucus in the middle, the epigastrium very sensible to pressure, the urine small in quantity and red, and the pulse strong and frequent.—Twenty leeches to the epigastrium, with directions to suffer them to bleed all night; lemonade with gum arabic; entire abstinence from food.

2d day. Tongue less red; pulse still strong; patient delirious during part of the night; head burning; eyes injected; headach very acute; urine still scanty and red.—Thirty leeches in the course of the jugular veins; head bathed with vinegar and water; mustard baths to the feet; emollient fomentations to the belly; lemonade with gum; entire abstinence.—At his visit in the evening, Dr L. found "every thing restored to order." Some pustules had appeared upon the face and trunk; and the stomach and brain had returned to their physiological or healthy condition. The abstinence was continued.

On the 3d day, the eruption was general. The rest of the treatment was "employed to facilitate the progress and termination of the cutaneous phlegmasia; which ran through its periods without impediment."

Case 2. A coloured man, of a nervous and sanguine temperament, aged 20 years, was treated by the author, for a month, for gonorrhœa. He had taken balsam of copaiva, in pills, for three days; when on the morning of the fourth, he was seized with the desire to vomit, flexion of his limbs, and pain in the head. While in this condition, by the advice of a friend, he took a glass of gin. This was followed, almost immediately upon its ingestion into the stomach, by a severe pain in that viscus, and a constriction of the pharynx. The other symptoms were aggravated, fever declared itself, and he was soon seized with convulsions so strong that four persons could hardly retain him in bed. At six in the evening, Dr L. found him with his face presenting an animated expression, his eyes haggard, making efforts to escape from his bed, the whole surface of his tongue extremely red, the epigastrium painful and of a burning heat, the pulse full and frequent.—Twenty leeches to the epigastrium, and an equal number on the track of the jugulars; directions to let the bites bleed all night; emollient fomentations to the belly; vinegar and water to the head; lemonade with gum; entire abstinence.

2d day. Tongue less red; expression of countenance improved; the furious delirium has subsided, but the patient now betrays occasional impatient motions. —Evening. All the cerebral symptoms have returned; an attack of epilepsy, with which the patient had been once previously affected at ten years of age; the skin dry. Bleeding of ten ounces from the foot; ice to the head; mustard baths to the feet; lemonade with gum; entire abstinence.

3d day; morning. Some eruptions on the face and thighs.—Continuation of the same means, except the bleeding.—The stomach and head were in good condition [supposed to be in the evening]. Vinegar and water were substituted for the ice.

4th day. A general eruption over the whole body.

6th day. The whole body "was a single wound." Emollients were opposed to the violent irritation of the skin; diet and demulcent drinks prevented the ill effects of the secondary fever; and the patient, at the end of thirty days, was in full convalescence.

After describing the above cases, Dr **Lere've'rend** remarks that he attended twenty-seven patients during the epidemic; that they were all treated according to the principles of the physiological medicine, and that he lost *none* of them. While his fellow physicians of the city, whom he describes as Brunonians, proclaimed that they had never met with so destructive a visitation of small pox; "he proved to them, by facts, that this was only the case in consequence of their bad treatment." His first success having attracted the public attention to him, various means were resorted to for the purpose of turning him into ridicule: among others, a caricature was made, in which he was represented, sitting upon a mass of ice, and bitten from head to foot by leeches. This was not productive of the effect designed: well informed persons inquired into the facts, and employed him, first for their slaves, and then for themselves. He informs us that, by those means, he was not long in becoming physician to the first houses in the city, among whom he includes his excellency, the bishop of Havannah; who employed him for an obstinate diarrhœa, "*after having perused the medical propositions of professor Broussais.*"

11. *Delirium from Intoxication, viewed physiologically.*—Dr **Francis Broussais**, surgeon in a French regiment, reports the following case: Jordy, a chasseur, took, early in the morning of Jan. 1st, a draught of four sous' worth of brandy, with a piece of bread. He breakfasted, as usual, about ten o'clock; at noon he went to sleep, and was awakened at one, to prepare to mount guard. At 4, P. M. this man, together with two of his comrades, drank three bottles of wine at dinner, without experiencing any inconvenience.

About 6 o'clock, he was affected with a general chill, horripilation, and tremors of all his limbs, and gave way to loud lamentations and tears. Next followed a high heat, with great irritation, and vertigo. He was then taken with convulsions; and when held by his companions, struggled with violence, not recognizing any of them. His delirium was of a marked character: he struggled to escape from the hands of his comrades, exclaiming that he saw Satan, with a legion of devils. Soon after, he had a more agreeable vision, in the shape of a girl to whom he had been attached; and with this kinder or more deceitful spirit, he held conversation for some time.

Dr **Broussais** saw him at 10 o'clock at night. The pulse was found full and frequent, but not hard. The face was injected, the head and abdomen hot. "Forgetting all the riches of the pharmaceutical arsenal," says Dr B., "I had recourse to the simplest of remedies, intending to open a vein, should that prove insufficient." Provided with a pitcher filled with "protoxide of hydrogen," Anglicé water, this erudite medicament was freely applied, both within and without; the patient being urged to drink, which he did to the amount of a pint in two hours, while cloths wet with the "protoxide" were applied to his head, and copious affusions made over his whole body.

After half an hour of this treatment, his delirium took the character of hilarity. He followed with his eyes the figures of his diabolical visitants, who were good enough to treat him to a grotesque dance. Occasionally, he manifested slight convulsive movements, followed by a species of exhaustion.

At one in the morning, he recognized his sergeant major, and, from that moment, recovered the use of his reason; he slept quietly, and awoke next morning with no other complaint than a slight sense of fatigue.

Our object, in extracting this and the foregoing cases, is not criticism, but the presentation of a lively idea of the practical application of the physiological rules of judgment. Where, however, strong temptation exists, we will not hold ourselves bound to refrain. This case is entitled "Delirium; hallucinations from gastric irritation. Recovery by the sole use of cold water." Dr Broussais considers the whole disease as produced directly by the stimuli applied to the nervous papillæ of the stomach. This he conceives he soothed by the application of the water; and adds that "the case comes in proof of this truth, which men, either superficial, or not entitled to esteem, endeavour to dissemble, viz. that the physiological doctrine teaches to economize the blood and strength."

We do not think that any American hospital, army, or navy physician would hesitate a moment to pronounce the above case to be one of the disease which has been styled delirium tremens, or mania a potu; and to express strong suspicions which might tend to the detriment of the character which the patient might bear for temperance: for we suppose the said surgeon could hardly be made to believe that Jordy had not taken more liquor than he acknowledged, on the previous and perhaps preceding days. The phenomena much more resemble, to our view, the effect of a *suspension* than of a *continuance* of the use of spirituous liquors. Under this view, we do think it fortunate that he refrained from the very natural resource of blood-letting. With regard to the effect of cold water, we do not see how the theory explains the employment and usefulness of the cold affusions. At the same time, the advantageous effect of these latter is confirmed by various practitioners.

12. *Hydrophobia generated by Heat.*—According to M. Unanue, at the period of writing his topography of Lima, hydrophobia was unknown in every part of South America. It appeared for the first time on the northern coast of Peru, in the summer of 1804, during a prolonged heat which sometimes raised the thermometer as high as ninety-nine and a half degrees of Fahrenheit. Almost every species of quadruped was affected with the disease, but particularly the dogs. It did not appear in the capital till 1807. In the village of Jea, forty-two persons died after having been bitten by mad dogs. On the north coast, hydrophobia occurred in several individuals without any bite. On a sugar plantation, several slaves are said to have fallen victims to it, after having eaten the flesh of an animal which had died of it. The epidemic disappeared in 1808. Several dogs were affected with it *twice;* but then their bites produced no dangerous consequences.—*Journal des Progrès;* from the Zeitschrift von Henke.

13. *Imaginary Hydrophobia.*—A curious case is narrated in the same journal, from the Giornale de Fisica, &c. in which a man was bitten by his own dog, whom he supposed to be mad. The dog then disappeared, and the master was seized with all the symptoms of hydrophobia; being unable to swallow any thing for four days. On the 5th, the dog returned home, and as soon as he fawned upon his master in his usual manner, the dreaded malady disappeared.

14. *Tetanus.*—M. Lepelletier, surgeon in chief to the hospital at Mans, has always found, when dissecting cases of traumatic tetanus, that there were very visible traces of phlegmasia in the meninges of the medulla spinalis, especially of the arachnoid at the roots of the nerves, and over the latter during their progress through the spinal canal. After a case of well characterized tetanus with

trismus, opisthotonos, &c. the sacral nerves, especially the sciatic, exhibited spots and numerous striæ of a vermilion red. When divided longitudinally, the nerves presented the same striæ; and when cut transversely, exhibited a multitude of bright red specks, which were the orifices of vessels. All these appearances existed in the neurilema, either in the portion covering the nerve, or in that which separated its different fibrillæ; but principally in the former. If one of these divided nerves was pressed, it gave issue to a considerable quantity of blood. The medulla spinalis, when simply laid bare, exhibited bluish striæ, and its tunics seemed distended by a fluid. The fibro-cellular membrane, (?) when incised throughout its whole length, contained, on the inside, numerous red patches, especially at the origin of the nerves: and the arachnoid was particularly remarkable for a vascular network very strongly marked.

The arachnoid of the brain was considerably injected, but presented neither patches nor adhesions.

In another case of tetanus, the disease was provoked by the left arm having been crushed; and the first tetanic symptoms were manifested in the arm. The neurilemas, principally of the ulnar and median nerves, were red, and inflamed quite up into the axillary plexus. The spinal pia mater and arachnoid showed many strongly marked inflammatory spots; but all these appearances were confined to the side which had received the injury. The strongest characteristic marks of inflammation were found in those parts of the medulla spinalis which were most in connection with the brachial nerves, the first point of departure of the neurilematic inflammation. It is exactly in those muscles whose nerves arise from the portion of the spinal marrow already pointed out, that the propagation of the disease was observed. Thus the respiratory muscles exhibited the tetanic stiffness in all its force; and the patient was several times on the point of suffocation from the mechanical suspension of respiration; while the trismus was only consecutive, and the jaws could be separated to the very moment of death.

The resemblance of these views to those of Dr SANDERS, of Edinburgh, will be noticed.

M. LEPELLETIER concludes, from these and other cases, that tetanus is an inflammation of the neurilema of the nerves and spinal marrow; (would he not include the trigeminus of the head in the case of trismus?) that traumatic tetanus spreads from the circumference to the centre, thus attacking the nervous tree from the branches to the trunk; that tetanic patients die from exhaustion, with the exception of those instances in which respiration is actually suspended long enough to produce a true asphyxia; and, finally, that the rational treatment for this formidable malady must consist in lowering the vitality of the injured organ in particular, and of the whole economy in general.

15. *Apoplexy of the Spinal Marrow.*—It is well known that the distinguishing mark of real apoplexy, viz. that which consists in an actual hemorrhagy in the brain, is considered by most of the modern pathological anatomists to be *palsy* of some part of the body. We have here a case of a similar affection in the medulla spinalis, reported by M. HUTIN, in the Nouv. Bib. Med. Fev. 1828.

A man of about 70 years of age died suddenly at the Bicetre, and the following particulars were collected respecting his previous condition. He was of a strong and robust constitution, a middle stature, and a sanguine temperament; his body was slightly curved forwards, and his legs were weak and stiff; so that he habitually employed a staff for the mere purpose of preventing himself from falling forwards. He could walk from his residence at the hospital to the city; but this required a considerable time; and frequently, notwithstanding the support of his cane, he found himself drawn forward in such a manner as to fall, unless he arrested himself against a tree or some similar obstacle. His superior extremities had also lost much of their strength and agility. To an understanding naturally confined, were added, in this man, an extraordinary difficulty and slowness in pronunciation, which existed to such an extent as to deprive him of social intercourse, and induce him to live alone. He never complained, and had an excellent appetite. The day preceding his death he spent in walking about the hospital;

he retired to rest at six o'clock, as was customary with him; and at four in the morning was found dead in his bed. It appears, from statements made to Dr H. that this man had suffered several attacks of apoplexy, and that this weakness of his limbs was the consequence of them.

Dissection, twenty-eight hours after death. Flesh firm; fore-arm and fingers bent and strongly contracted; legs extended and stiff. On the incision of the teguments of the cranium, a large quantity of black blood escaped; another portion of a similar fluid ran out of the cavity of the cranium when opened Dura mater thin and transparent; sinuses engorged with blood; pia mater injected; brain possessing considerable firmness, except at the posterior and external part of the left hemisphere, where there was a slight superficial softening.

In each of the corpora striata there was a cavity, large enough to contain an almond, lined by a cellulo-vascular coating, infiltrated with a citron-coloured serum. The upper parietes of these spaces appeared to be formed by the arachnoid lining of the ventricle, thickened. The cerebellum presented nothing remarkable; but the pons Varolii was slightly softened. At the opening of the *spinal canal*, there escaped a large quantity of black blood; the meningeal veins were distended with that fluid; the arachnoid cavity contained much serosity, in which were floating a few drops of a fatty liquid, and some bubbles of air. In the arachnoid of the lumbar region, were a number of cartilaginous plates, of a bluish white, some of which exhibited, in their centre, a small bony point, projecting into the cavity, opaque, and composed of radiating fibres. The veins of the medulla spinalis were distended, and, particularly at the lower part, were very much dilated and flexuous; the imperfect partition which penetrates into the anterior and posterior fissures was strongly injected. The medullary substance possessed its natural consistency; it was grooved, in every direction, by numerous little vessels.

Between the origin of the fifth and sixth pair of cervical nerves, was found a small bloody effusion of the size of a pea, which had, in this place, destroyed the cineritious commissure. And at the elevation of the fourth dorsal pair, existed another much more considerable effusion, which had destroyed the continuity of nearly all the nervous matter of the medulla, whether white or cineritious. The clot of blood was as large as a good sized hazel nut, and a little elongated in the direction of the length of the canal. Adjacent to these two effusions, the cerebral matter was slightly softened, and infiltrated with blood.

The lungs were gorged with blood, and the cavities of the heart distended by large clots. The intestinal canal was sound.

16. *Atrophy, Hypertrophy, and Induration of the Spinal Marrow.*—Dr HUTIN has published numerous cases and reflections respecting these diseases, from which are drawn the following inferences:

Atrophy may take place in the spinal marrow from various causes: sometimes it is from the entire absence of muscular motion, as in persons affected with hemiplegia or rickets, and the debility of old age; sometimes again, it seems to arise from some cause inherent in that organ, and which it has as yet proved impossible to discover. Very frequently, it is the result of a gradual and long continued pressure exerted on the organ.

It is, in general, not difficult to establish the diagnosis of atrophy of the spinal marrow. After having made out the existence of a more or less complete paralysis, which has developed itself in a gradual manner, without any other complication than coldness and wasting of the extremities, it will commonly be found that the previous history of the case will furnish sufficient means of recognizing and distinguishing the disease. It may easily be imagined that this must be a malady of unfavourable prognosis. If it affect only the inferior portion of the spinal cord, it suffers life to be prolonged under circumstances almost insupportable; depriving the patient of the use of his lower limbs. If, on the other hand, it occur in the cervical portion or at the cranial extremity, it sooner or later becomes mortal, after producing palsy in the arms, the organs of sense, &c.

Medicine is absolutely without resources against the spinal atrophy; nothing being able either to prevent it, arrest its progress, or effect a cure, with the single exception of that which occurs as a consequence of the entire repose of the body. In this case, it seems that exercise of the limbs will, under some circumstances, impede its full development. Physicians who attend a large number of patients will undoubtedly have remarked, that those paralytics who submit themselves to their unfortunate destiny without a struggle, and suffer themselves to become a prey to apathy or chagrin, never obtain any relief; while those who exhibit energy, moral and physical, and oppose, in a manner, their will to the disease, often finish by recovering the use of their limbs, if not entirely, at least in a great measure.

We are not as yet possessed of a sufficient number of well recorded facts relative to the *hardening* and *hypertrophy* of the spinal marrow, to enable us to trace a complete and exact history of these two affections: from the observations of our author, however, it is admissible to draw the following conclusions. 1. Induration and excessive growth of the spinal marrow are of much more common occurrence than is generally supposed. 2. Their causes are still very obscure. 3. Their nature does not seem to be in all cases the same: and it seems, in fact, that, in some circumstances, they are really the result of excessive nutrition; while, in others, they evidently depend on a chronic phlegmasia. 4. The induration and hypertrophy most generally exist together; yet they sometimes occur separately. 5. When these two states were found simultaneous, the patient had previously exhibited a more or less considerable increase of the sense of touch in those parts which correspond to the diseased portion of the medulla; besides convulsions, spasmodic contractions, chorea, epilepsy, &c. &c. and a peculiar paralysis, characterised by the complete abolition of the force and energy of the muscles, coinciding, nevertheless, with *an entire freedom of motion*. 6. Hypertrophy, when not complicated, seems only to produce a greater or less increase of the sensibility of the nerves of touch; while all the phenomena just enumerated which occur in the muscular apparatus, have uniformly been found to co-exist with hardening. In the latter case there also existed a remarkable diminution of sensibility. 7. These two organic alterations are not necessarily mortal: all the patients having died under the influence of other causes. 8. Hardening and hypertrophy of the spinal marrow have always proved to be beyond the curative resources of medicine.—*Ibid.*

IV. THERAPEUTICS, MATERIA MEDICA, AND THE PRACTICE OF MEDICINE.

17. *Dysentery.*—Dr Meyer relates, in Hufeland's Journal for April 1827, his success in the treatment of epidemic dysentery by the use of the nitrate of soda. His mode of administering it is as follows:

R.	Nitrate of Soda, . .	ʒss. to ʒi.
	Common water, . . .	℥viii.
	Gum-dragon, . .	10 grains.

Make a solution—of which the dose is a table spoonful.

This salt, which has a laxative and diuretic effect, cured, or at least mitigated very much the intensity of the disease in the space of twenty-four or forty-eight hours.

Though the dysentery in question was presented under an inflammatory type, it was rarely deemed necessary to have recourse to blood-letting, either general or local.

As dysentery has already appeared within the circle of our own observation and practice, and will no doubt, be engaging the attention of many of our readers at the time when these lines meet their eye, we deem it not improper or irrelevant to proffer a few remarks on the treatment of this ever formidable malady. Without any great pretensions to age or experience, we may be allowed to say, that for twelve years past, we have had frequent opportunities of witnessing and treating dysentery, under many varieties of climate and locality. Were we to be influenced by results alone, we might dwell with some complacency on the success of our practice; but as this, like experiment, is occasionally fallacious, we shall merely state the course which we have more usually pursued. Allowed from the very beginning of our career to act as free agents, we began with the use of the lancet, as well on the competent authority of classical medical writers, as on what seemed to be called for by the symptoms of the disease; and we can safely aver, that in no instance have we had reason to regret that free venesection had been practised, whilst we have, from time to time, had to reproach ourselves with procrastinating it in favour of pharmaceutical means. Of these latter we have used emetics, purgatives and opiates in the successive stages of the disease. We have at times, and indeed not unfrequently, placed great reliance on calomel, at first alone, and subsequently combined with opium, to alternate with or be followed by castor oil, or the oleaginous mixture. If we have not seen such notable differences to result from the operation of such various agencies, for to say various modes of practice would hardly be correct, we think the cause must be found in our having associated the lancet with all of them. We the more incline to this opinion, from having of later years obtained all that we could have once promised ourselves, by a restriction to venesection, general and local, and the use of diluent and demulcent drinks, with occasionally an opiate, by which we mean an opium pill or a Dover's powder. Of free cupping or leeching the abdomen, we can speak in terms of unequivocal praise, as an adjuvant to the lancet, finishing what the latter had begun, and removing the pain, tormina and tenesmus expeditiously and permanently. The effect of one operation, though often insufficient, will be of such a nature as to encourage to a repetition until every wish be realized.

The chronic form of dysentery, consisting chiefly of protracted and habitual morbid secretion from the mucous surface, which in the earlier period exhibited the more distinctly marked symptoms of phlogosis and tendency to ulceration, demands in its treatment certain modifications of that pursued in the acute stage. We say modifications: we do not mean essential differences. If the experience of impartial men has shown that astringents and tonics separately or combined, or joined with stimuli, are injurious, if not decidedly pernicious, in the first stage of dysentery, we are yearly acquiring conviction by similar evidence that these means are of little or no avail in the second or chronic. What large bleedings had accomplished in the former, small or local bleedings will ensure for us in the latter. If mercury and opium have benefited the one, they will not be without a salutary effect on the other. Here, however, we must be a little more explicit. It will have been inferred, and correctly by our readers, that we are disposed to abandon to the reprobation of the physiological school, the majority of the drugs in vogue for acute dysentery, and rest our hopes on general and local blood-letting, diluents, and an occasional opiate. But we are not yet prepared from the evidence, chiefly indeed on one side, which has been furnished to us in our practice, to make a similar surrender in chronic dysentery. We grant that we have done so in some cases with entire success and to our satisfaction; but we are required in a spirit of impartiality to say that other means have had as signally a beneficial effect. The chief of those we now proceed to mention.

Having studied, very early in our medical noviciate, the work of Mr ABERNETHY, in which the blue pill is so highly lauded in all the disorders of the digestive canal, we were naturally prepared to give it a trial in chronic dysentery. Circumstances having placed us for a time in a tropical climate, our attention was of course more specifically directed to the subject. What CLARKE had pointed out, in his work on long voyages, regarding the good effects of mercury rubbed down with gum arabic in dysentery, was made to us more convincing by conversations with a very intelligent and experienced resident surgeon, Mr PEARSON, of Canton, who expressed his decided preference for the blue pill, in those diseases in which mercury was called for. Opportunities were not wanting to test the correctness of this opinion, and we can now, after ten years experience, declare our firm reliance on the salutary operation of this formula, and of its superiority in chronic dysentery, over the other preparations of the metal. Cases of a protracted and obstinate character, have yielded to the blue mass in doses of five grains, once, twice, or thrice daily, with no other addition, either medicinal or dietetic, than rice or barley water, especially the former. If the occasion, or subject required additional enforcement of what we have stated by collateral proofs deduced from the operation of mercury, especially the blue mass, on the mucous membranes generally, or the recommendation of the medicine by approved writers on dysentery, such as BALLINGALL and others, we could readily present it with some force. But our purpose was to give what we conceived to be a satisfactory outline of the treatment of dysentery, and having accomplished this, we close our remarks for the present.

18. *Mercury absorbed into the System.*—The analysis lately made in the chemical laboratory of the Faculty of Paris, of the mammary and salivary glands, mesentery and large intestines of a young woman who had fallen a victim to puerperal peritonitis, and who during the disease had been subjected to large mercurial frictions (12 oz. 5 dr. in the course of twelve days), exhibited mercury in its metallic state in these organs. This result, though opposed to the assertions of many chemists and a great number of physicians, confirms the researches of FOURCROY, DUMERIL, ORFILA, and CRUVEILHIER, who have discovered mercurial globules in the bones, cerebral substance, and nerves; and no longer allows us to doubt of the fact, notwithstanding the warmth with which it has been at different epochs contested, and the ridicule attempted to be thrown on it.—*Journal des Progrès, &c. Vol. VIII.*

19. *Chronic Bronchitis cured by the External Application of the Acetate of Morphia.*—The Journal des Progrès copies from the *Methode Endermique*, of LEMBERT, an account of a cure of chronic bronchitis, in which the usual anodynes and pectorals, with a blister to the arm, were found of no avail. To relieve a constipation of eight days duration, five grains of powdered aloes were sprinkled over the vesicated surface, which after the lapse of ten hours gave rise to several alvine discharges and colicky pains. The effect thus produced encouraged Dr LEMBERT to make trial of a sedative remedy on the same surface. Accordingly on the 14th of March 1824, half a grain of acetate of morphia, in powder, was applied: this was soon followed by slight pricking felt in the sore; in a quarter of an hour afterwards the tracheal tickling was moderated and the cough and oppression diminished. On the 17th, an hour after the application of a grain of the acetate of morphia, the patient experienced a feeling of general ease and comfort, and enjoyed the luxury of sleep which had been so long denied her. The medicine was continued until the 22d, with an increase of the dose to two grains; and so decided was the amelioration of the disease that the cough had disappeared. Wishing to be assured that the subsidence of the catarrhal symptoms was entirely dependent on the acetate, its use was discontinued on the 23d. All the former evils promptly returned, with the addition of cold sweats, chills, and faintings. The use of the medicine was resumed on the 24th, in doses of three grains, and with so happy an effect, that, by the 5th of April, the patient was beginning to recover her flesh, and no longer was troubled with either cough or expectoration. Whenever the suspension of the remedy was attempted, the effect was invariably the same. That the imagination of the patient had no share in the cure was evidenced by frequently deceiving her either in regard to discontinuing the medicine or to using it, without the results being at all at variance with those before obtained. The dose was finally raised to four grains, and from this time forward, the condition of the patient became daily more satisfactory, evinced in a return to her former plumpness, with some degree of freshness of complexion; but still the acetate was continued. Towards the end of May, she felt so well and believed herself to be so completely cured, that she asked for leave to go home.

20. *Endermic Medication.*—For some interesting particulars respecting en-

dermic medication, of which the preceding history is an interesting example, and the best means of using it, our readers are referred to Vol. III. pp. 189—91 of this Journal. The following additional information is from the *Journal des Progrès*, which in its turn derives it from the work of Dr LEMBERT entitled, *Essai sur la Methode Endermique*. This method consists in applying active medicines to the surfaces of the cutis denuded by vesication, or to that of the subcutaneous tissues. From the means capable of producing vesication we must exclude boiling water as uncertain and dangerous. The pain so generally caused by vesicating substances may be greatly mitigated by surrounding them with emollient cataplasms. It is better to discharge the serum through an opening made in the phlyctena or raised epidermis, into which also the pharmaceutic agent is to be introduced. The absorption is thereby more active. During the dressings we must be careful to remove the suppurative coat formed on the surface, for which the chloride of lime answers very well. When it can be accomplished the medicines should be reduced to an impalpable powder; and if their immediate action be too irritating, they should be mixed with gelatin or lard, directing of course, in such cases, a corresponding quantity of the compound. Liquids ought to be slowly applied, drop by drop, and resinous bodies spread in the form of a plaster. The dose of medicines thus administered should be gradually increased, then be continued at their maximum for a while, and finally decreased. Their effect is more energetic in proportion as they are applied near the nervous centres; and more benefit will be obtained by their application to a number of small surfaces than to a large one which should equal all the others in extent. The accidents, that is the unpleasant and unforeseen or alarming symptoms following this kind of medication, may be promptly arrested by carefully cleansing the vesicated surface, or by compression or dry cupping, or finally, by the application of a substance possessing properties of an entirely opposite nature from that of the offending one. Thus tetanus brought on by two grains of strychnia has yielded to two grains of the acetate of morphia. Medicines applied to the cutis give rise to two well marked effects. The first, generally irritating, consists for the most part, in an itching or burning sensation, accompanied by redness and injection of the denuded surface. The second, as various as are the medicines applied, is evidenced at the expiration of about ten minutes, or one, two, or three hours after the application: it is usually displayed in a feeling of heat spread from the denuded part towards the nearest viscus, whence it is diffused throughout the animal economy, in following the course of the principal vascular and nervous trunks. The absorption seems to be more active at the internal than the external side of a limb, and at the anterior than posterior surface of the trunk; more in persons of a thin delicate skin than in those who have it of a dark colour, dry, dense, scaly, or hairy. It takes place with most effect in the evening, or at night after a bath, and when the digestive passages are empty. In fine, it seems to be more energetic in damp than in dry weather, and in hot than in cold seasons.

21. *Intermittent Fever.*—Among the many varieties of this disease we every now and then meet with some of a more peculiarly anomalous character; such as where only a particular part of the body is affected, and in which alone the three stages cold, heat and sweating are gone through. The *Journal des Progrès*, &c. Vol. VIII. furnishes us with several of these cases. In one, the disease affected only half of the body in a vertical direction, so that the median line was the boundary between the healthy and diseased parts. In another, recorded by M. DUVAL, the subject of which was a soldier in the military hospital at Brussels, the cold stage of the fever, which was of a quartan type, was accompanied by a violent spasmodic contraction of the lateral half of the body, most remarkable about the eyes and face, and communicating a hideous expression to the patient. But what rendered this case still more singular, the two halves of the body were, each in its turn, thus attacked alternately. We read in the *Revue Medicale* for 1814, of a man, who, from having slept in a room freshly plastered, was seized with intermittent fever in that side of the body corresponding to the

wall near which his bed was placed. M. D'AUXIRON relates, in the *Journ. de Med. Tome* XXIV. p. 60, a case of intermittent fever, in which the lower half of the body, or its subdiaphragmatic was alone affected. As a parallel to these is the history of a patient given by MORGAGNI, in which jaundice supervened on hemiplegia of the right side, after an attack of apoplexy The jaundice only affected, however, the paralysed side, and this in so distinct and well defined a manner as to contrast most singularly with the left, which even to the nose preserved its natural colour. Cases are on record of persons, who, all their life time, only perspired from one side.

Passing from the curiosities of the subject to what most of our readers will consider as of paramount importance, viz. the means of cure; we now lay before them the following note from Dr C. D. MEIGS, on a formula, the administration of which in the practice of the writer, and many of his medical brethren, has been productive of the very best effects in preventing a return of the paroxysms of intermittent fever.

"In the autumn of 1823, says Dr M., I had a patient, Mrs J. C. who laboured under a severe tertian. I had vainly endeavoured for many days to cure her by means of various preparations of bark. When she recovered she informed me with much hesitation, that I had not cured her, but her disease had yielded to a medicine given by her kind neighbour Mrs C. As I had been much embarrassed by the obstinacy of the case, I called on the latter to make some inquiries, and was informed she had taken the liberty of interfering, because she had *never* been disappointed, in a great number of cases, where she had procured the medicine, either for the suffering poor, or for her friends, whose attacks had proved exceedingly obstinate *under* common modes of treatment.

"The formula was as follows:

Red bark in powder, ℥ss.
Theriac of Andromachus, ʒi.
Lemon juice, ʒii.
Port wine, ℥iv.—M.

Of this mixture, one wine glass full was given at a dose every second hour during the apyrexia. Mrs C. assured me that she knew of more than one hundred persons who had taken it with success. From these representations, made by one on whose candour and good sense I could rely, I was induced to employ the formula in a great many cases; and the comparative results of my practice with this and various other formulæ for exhibiting bark, and its salts, have constantly impressed me with the conviction of its superiority over them all, as a general resort. Having communicated my success to several medical friends, I am confirmed in my opinion by their confidence in it, which seems to be not inferior to my own. I may take the liberty of mentioning that some of our professors have employed it very largely and successfully: indeed it has become here, merely on account of its excellence, a very common prescription.

"In recommending this mode of using bark, I hope to be exonerated from any imputation of encouraging an empirical practice; for I conceive that no such charge can be made against the employment of bark in intermittent fever. The physician must decide the proper time for giving the medicine. I will only further say, in relation to it, that I employ it in the first complete intermission.

"If any of the readers of this Journal should deem it proper, after what I have said, to prescribe bark in the manner above recommended, I have little doubt of their obtaining a success correspondent to that which in a great number of instances I have met with."

22. *Tetanus cured by the External Application of the Acetate of Morphia.*—We are furnished with two cases, one of traumatic, the other of idiopathic tetanus, in the essay on the endermic method by Dr LEMBERT, already referred to. In the first, that of a female, there was trismus, rigidity of the muscles of the neck; abdomen tense, hard as stone; legs convulsively flexed, and without the possibility of extending them, the eyes fixed, cheeks drawn back; the pulse was hard and frequent. Bleeding of twelve ounces, thirty leeches to the anus followed in

two hours by a warm bath of an hour's duration, and the use of mercurial frictions on the neck and legs, with a blister and sudorific drinks, were not productive of any marked effect. The endermic plan of Dr Lembert was then had recourse to, and a quarter of a grain of acetate of morphia, mixed with a minute portion of cerate, was applied to the vesicated surface, at ten o'clock in the morning of the the second day of the disease. The trismus ceased completely, but the rigidity of the neck still persisting, the dose of the acetate of morphia was doubled at eight o'clock in the evening. At eleven the functions were restored to their normal state, and the night was passed in a tranquil manner. The tetanus was in this instance consequent on an ulcer over the external ankle, in a woman aged twenty-eight years, of a scrofulous habit, in the hospital of Salpètriere.

The second case of idiopathic tetanus occurred in a female in the Salpètriere, with incurable herpetic eruptions on the inner side of the upper and lower limbs. The tetanic attack was immediately consecutive to fright and syncope caused by a companion with whom she was walking, falling into her arms in an epileptic fit. At eleven o'clock in the morning following her attack, marked by general rigidity and irregular contraction of the muscular system, a small blister was applied to the back of the neck, and removed at three o'clock in the afternoon. The vesicated surface was then sprinkled with a quarter of a grain of the acetate of morphia. At six o'clock, the trismus had disappeared, but the other symptoms still persisted. Another quarter of a grain of the medicine was applied, and at ten o'clock the fore arms could be extended. It was only during the course of the night that the muscles of the neck, face and eyes regained their customary mobility. By seven o'clock the following morning, the disease had entirely disappeared.

23. *Borax employed to cure Furfuraceous Herpes.*—In Hufeland's Journal for October 1827, we are told, that a Dr Reinhardt of Mühlhausen (Prussia) had had a furfuraceous herpes on the dorsal surface of both hands, the skin of which presented scattered red spots. He determined on trying the supersaturated borate of soda, according to the method of Hufeland, in the dose of half a drachm to an ounce of distilled water. During the first few days in which he made use of the solution, each successive application of it was followed by a stinging sensation and redness of the affected parts, so as to require a suspension of the remedy until the irritation thus produced had been removed. He then resumed it without any farther inconvenience. The herpetic eruption gradually diminished and at last entirely disappeared before the half drachm of the borate had been all used.

Some time after this two persons affected with furfuraceous herpes were treated by Dr Reinhardt in the same manner as that just detailed, and with the like success.

24. *Herpes of the Organs of Generation cured by the Hydrocyanic Acid.*—The Journal already quoted in the preceding article contains, in its number for March 1827, the account of a woman aged fifty years, affected with herpes about the genital organs, productive of excessively painful pruritus. Dr Schneider prescribed lotions of an alcoholic solution of hydrocyanic acid, in the proportion of a drachm and a half or two drachms of medicinal prussic acid to six ounces of alcohol. The patient was completely cured by this means in a period of fifteen days.

The same solution in six ounces of distilled rose water was attended by the like results in another female, aged eighty-four years. Dr Schneider reports three other cases in which he was equally successful with the remedy.

25. *Balsam Copaiba without Taste or Odour.*—The *Annales de la Medécine Physiologique*, for September 1827, give an account of the success of M. Salle, who is both a doctor of medicine and an apothecary, in depriving the balsam copaiba of its unpleasant taste and smell, by which it so often nauseates patients for whom it is prescribed. We are told that the strength and medicinal properties of the article are not at all impaired by the process.

26. *Volatile Oil of Copaiba.*—We have lately seen, says the editor of the London Medical and Physical Journal (March 1828), some volatile oil obtained by distillation from copaiba, by Mr BATTLEY of Fore Street. It agrees in its physical and chemical properties with the oil of turpentine, excepting in its smell, which is somewhat different. There can be but little doubt that on this oil the powers of copaiba as a remedial agent depend; and if this be the case, the resin of copaiba, concerning which so much has been lately said, can have little or no activity, in consequence of the oil having been driven off. Several trials with the resin have proved the correctness of this inference. The volatile oil of copaiba requires a heat of about 600° F. to obtain it, and is then procured only in very small quantity.

27. *Iodine successfully used in the Treatment of Enlargement of the Liver and Spleen.*—Dr MILLIGAN at the Royal Universal Infirmary for Children, has succeeded in curing cases of enlarged liver and spleen, by the administration of the tincture of iodine (proportions not stated), after mercury had been employed in vain. Seeing the communication of Dr MILLIGAN under the head of "original papers" in the London Medical and Physical Journal, we were on the point of passing the article to its credit, when in some editorial remarks we find it mentioned, parenthetically, that the history of the cases is from the Medical Repository. By original papers most journalists understand essays or reports never before published.

28. *Nævus Maternus cured by Vaccination.*—We copy the following from the May number of the London Medical and Physical Journal.—Jane M'Gibbon, at three months, had, on admission into the Royal Infirmary, Glasgow, a roundish tumour, nearly as large as a sixpence, on the right side of the chin. It was considerably elevated above the surrounding skin, had a purplish colour, and appeared to be formed of blood vessels. At birth the tumour was only the size of a split pea.

Around the border of the tumour, as well as all over its surface, minute punctures were made, and vaccine lymph freely applied. In ten days thereafter, the whole disease was involved in one pustule; but when this became incrusted and was thrown off, there still remained a dark coloured, prominent, and diseased surface. Another suppuration succeeded, and a third; when, at the end of five weeks, a complete cure was effected, no trace of the disease remaining, nor mark, further than what follows vaccination on a healthy part.

This cure was hardly completed, when another little child was presented in private practice, with a nævus advancing rapidly and occupying the middle and edge of the upper eye-lid. The same process was followed with very similar results. A cure was effected, but after a very tedious festering and ulceration. Indeed it appeared evident that nothing further is required to effect a cure than to institute a process of suppuration, by cow pox, by caustic, or the like. As the most gentle of all these means, vaccination is entitled to a preference. But, when this has been used in the common way, and for the ordinary purpose, and before a mark of this kind has excited much attention, the writer is inclined to think that an antimonial plaster would constitute by far the best substitute. He regrets that no case has yet occurred to give him an opportunity of making the experiment.

Before these two cases fell under his care, a child was admitted into the Infirmary with a nævus about the size of a large almond, occupying the red part of the under lip. It was clipped away, not by triangular excision, but along the line of the lip. Three vessels required ligature, but in ten days the part was covered by a healthy cuticle, and no disfiguration occasioned. He is satisfied that this mode should more frequently be employed in extirpating cancerous lips. He has frequently performed it successfully in extensively diseased cases. It is uniformly practised by Baron DUPUYTREN.—*Dr* YOUNG, *in Glasgow Journal.*

29. *Muriate of Iron in Gastro-Malacie or Softening of the Stomach.*—The London Med. Repository and Review for May last furnishes the following article.

In an epidemic disease which prevailed at Heilbrun in 1825, Dr Pommer lost two children. Post mortem examination proved the affection under which they fell to be gastro-malacie. Some time afterwards he was called to attend two other children with the same disease, and from what he had learned of the practice of Autenreith with respect to the use of the muriate of iron in those violent diarrhœas which so frequently attend the severer forms of typhus fever, he was induced to employ it in these cases.

Case 1. This was an infant six months old. It was brought up by hand, and its health had been very good from its birth. It had been ill eight days. No food would remain on its stomach. It had six, eight or nine evacuations daily. Sometimes they were thin and aqueous, sometimes coagulated and pultaceous; they were pale coloured and extremely offensive. It was fretful, uneasy and seldom quiet; the face was very pale; it frequently thrust out its tongue. It had a haggard countenance. The state of the abdomen appeared natural to the sight and touch; not contracted by spasm nor distended, not too hard, too soft, nor too sensible. It had great thirst, a dry hot skin; the feet were sometimes cold, respiration was easy; the pulse was accelerated but not too hard, the sleep was short, disturbed, half comatose, with frequent startings, the eyes rolled; much groaning and restlessness.

The treatment was commenced by forbidding all aliment, except two spoonfuls of warm milk twice a day. A cataplasm made of red erichma bark, wild chestnut, willow bark, and aromatic powder, of each an ounce, was applied to the epigastrium. Cold applications were ordered for the head, and sinapisms for the legs. Two tea-spoonfuls of the following mixture were given every hour:

Take of Strong decoction of mallows-root, ℥ij.
Gum acaciæ in powder, ʒij.
Muriate of iron, gr. x.
Syrup of mallows, ʒviss.—Mix.

The next day twelve and a half grains of the muriate were given in the same vehicle: during these two days the child vomited but once, and passed three green stools. In the morning of the third day it had two hours sleep, which it had not enjoyed for eleven days. It appeared much improved, but this amendment was not of long continuance, for the disease returned in the afternoon. On the fifth day the vomiting ceased, and only two alvine dejections took place. It now showed inclination for food, yet nothing was allowed beyond its milk. Its sleep was still restless, with occasional groaning and rolling of the eyes. The temperature of the skin became natural. The regimen and external treatment were continued; but as all the symptoms were gradually receding, the applications of cold to the head were less frequent, the muriate was now given every two hours, and on the eighth day of the treatment, the sixteenth of the disease, the astringent aromatic cataplasm was discontinued. The appetite not being satisfied by milk alone, light farinaceous food was given three times a day. At length convalescence became more and more established, and the child was restored to perfect health.

Case 2. Another child, four weeks old, presented the symptoms of incipient gastro-malacie. It was cured by the internal use of the muriate of iron, without the aid of the external applications which were employed in the above case. —*Heidelberger Klinische Annale,* 2 *B.* 2 *H.*

30. *Tetanus cured by Enemata of Tobacco.*—The patient was a shoemaker who had wounded the index finger of the right hand with his awl; but so slight was the puncture that he paid very little attention to it. Eight days afterwards Dr N. was sent for, and found on his arrival, that the man had all the symptoms of tetanus. He began the treatment of the case by scarifying the wounded finger and covering it with red precipitate and an emollient cataplasm: laudanum was given internally, and friction with mercurial ointment practised on the nucha

and jaw. He left word also that if all these measures failed of relieving, an infusion of two drachms of tobacco in a pint of boiling water should be given as a clyster. It was given accordingly at eight o'clock in the evening; the patient felt some uneasiness in half an hour after; but the stiffness of the muscles soon began to diminish, and with a little assistance he could carry his head forward; the contraction of the arms and thighs ceased, though the jaws still remained in close contact. The unfavourable symptoms having returned in a short time, a fresh enema was given, with the same effects as at first, but of shorter duration. The remedy was repeated however, until a cure was accomplished. The simultaneous use of bark, wine and laudanum was thought by Dr N. to have produced no apparent diminution of the malady, the symptoms of which yielded as soon as the patient had taken the enemata of tobacco, which always occasioned some distress, and occasionally vomiting, but rarely fæcal discharges. He thinks that the tobacco ought to be employed with great circumspection in weak subjects; whereas in those of a vigorous and robust make, with a full, strong and tense pulse, when blood-letting would seem to be indicated, it is a very valuable remedy.—*Journal des Progrès, &c. from a German journal.*

31. *Poisoning by a Tobacco Enema.*—To enforce with more effect the salutary caution given at the conclusion of the preceding article we subjoin the following. Elizabeth Payne, at Bethnal Green, near London, was persuaded, by an empiric named Gabriel Ore, to make use of a clyster made by infusing an *ounce* of tobacco in water, as a cure for worms. Soon after its administration, she was seized with horrible convulsions, and in fifteen minutes expired. On the testimony of a physician, Dr UGAR, that a sixteenth part of the above quantity of tobacco would be sufficient to cause alarming effects and even death, the quack who recommended this *specific* was committed to Newgate, after a verdict of manslaughter had been found against him.

32. *Dry Cupping and Ligature.*—M. MERAT, in a note to the Academy of Medicine of Paris, respecting the bites of serpents, stated, that it would appear, by a quotation from pere LABAT, as if for the last hundred and thirty-four years, the remedy of cupping, and the ligature, had been successfully employed against the bite of the trigonocephale snake.

33. The Revue Medicale, for February, contains a note from Dr J. B. BOUSQUET, giving the results of cupping, after vaccination had been practised. It would seem that although, in many instances, the application of the cups prevented the vaccine virus from taking effect, yet in others it had no such agency. Out of 200 punctures on nineteen subjects, there were 119 regular vaccine pustules; of which forty-three, or about a third, were raised and developed under the cups, and in despite of their use. Our readers will find these facts somewhat at variance with the opinions and inferences so confidently advanced and implicitly received, on the occasion of the experiments of Dr BARRY on poisoned wounds.

34. *Chlorine as a Preservative from, and Cure for the Plague.*—M. Leseps, the French Consul at Aleppo, has recently sent home an account of the great success attending the use of the chlorides, (as recommended by M. LABARRAQUE) for the prevention and cure of the plague. It is improperly, says the editor of the Journal General de Medecine (Dr A. N. GENDRIN), that we apply the term chlorides (chlorures) to what are in fact solutions of chlorine in alkaline waters. They operate as chlorine would do, with the additional advantage of rendering this latter more active than it would be in the gaseous state.

35. *Inoculation for the Plague, and the Protecting Powers of Olive Oil.*—While on the subject of the plague we must apprise our readers of the result of experiments performed by a Spanish physician, SARAFINO SOLA, at Tangier (Barbary) noticed in the Journal des Progrès. He obtained leave from the government, while the plague was raging at Tangier, to inoculate some Spanish deser-

ters, to the number of fourteen, with equal parts of sweet oil and the matter from pestilential buboes and carbuncles. The pus was taken from persons in whom the disease exhibited all its worst features: it was introduced by punctures with the lancet in twelve places on each individual, viz. three on each arm-pit and three on each groin. In seven of the fourteen submitted to the experiment, there was not the slightest trace of either local or general disturbance. In seven others, there was experienced, between the fourth and tenth hour after the inoculation, some inconvenience, such as in three a small pustule at the groin, in another a carbuncle on the seat; in three others some symptoms of general disorder and slight irritation in the vicinity of the incisions. They were then kept together in an apartment, and secluded from all intercourse with other persons; and as the symptoms had only just begun to manifest themselves, the oil was used by means of frictions externally, and swallowed. All recovered their health perfectly, without any other means being employed, most of them in twenty-four hours, the others in the course of the following day. Since that time they have enjoyed good health though daily exposed to infected persons.

Our readers will, after perusing the above account, if they have no prepossessions on the subject, be inclined to ask whether these facts go to prove how little the plague is contagious, or whether they are evidence of the prophylactic and curative powers of olive oil in the malady. How far also, we would ask, were the privations to which these deserters had been subjected, and their previous restricted diet, instrumental in rendering them little susceptible to the influence of epidemic and contagious diseases.

M. Graberg, the Swedish consul-general, who communicated the details respecting the inoculation, says "that the plague is never communicated through the medium of the open air, but always by the actual contact of infectious matters. People may be seized with the plague as often as they are exposed to the contagion, which could be well verified, during the last fourteen months, in many who had the disease three or four times in succession. To be infected requires a certain predisposition wanting to the half of mankind. Finally, cold and certain dry and strong winds diminish the force and limit the extent of the disease."

V. MEDICAL JURISPRUDENCE.

36. *Homicidical Monomania.*—It is now, says Dr Georget, from whom we propose again borrowing, more than thirty years since the celebrated Pinel proved that a sanguinary impulse may be accidentally developed in the most upright man, and lead him irresistibly to the most terrible excesses without reasonable motives. And yet this truth, clearly demonstrated by facts of the most unequivocal nature, has seldom had its due weight before the tribunals of justice, and is even still rejected by the judges as a fatal error.

The existence of an extraordinary variety of homicidical monomania, or of partial insanity prompting to manslaughter, has been more particularly contested. We can meet with few who are inclined at first to admit, that by an exclusive lesion of the sentiments or of volition, and without any disorder of the intellect, a man may be in such a morbid state, that he is impelled with violence to shed the blood of his fellow creatures, to destroy the dearest objects of his affections.

Dr Esquirol was for a long time inclined to doubt the possibility of homicidical monomania *without delirium*, or of that variety called *reasoning madness, mania without delirium, maniacal fury*; but of late years he has had occasion to become fully sensible of the correctness of Pinel's distinctions, and has given the subject a most profound examination, with the convincing addition of numerous pertinent facts. He now tells us that homicidical monomania is presented under two distinct forms. In the first, the murder is committed from a thorough but delirious conviction of its justness; by the exaltation of a bewildered imagination, by false reasoning, or by the passions in a state of delirium. The monomaniac is excited by an avowed and unreasonable motive, and always presents sufficient evidence of a partial delirium of the intellect and affections.

In other instances, constituting the second form of the disease, the homicidical monomaniac presents no appreciable alteration of the intellect or affections. He is led by a blind impulse, by *an idea*, by *something indefinable* which impels him to kill; and even though his conscience warns him of the horrible character of the act which he is about to commit, yet volition being impaired is conquered by the violence of the bewilderment. The man is deprived of his moral liberty, he is a victim to partial delirium, he is monomaniac, he is mad.

Partial madness has not always, as its characteristic, the alteration of intellect. Sometimes the affective faculties are alone injured; at times the only disorder evident is in the actions. This is what authors have called *reasoning madness*.

In illustration of the first variety of homicidical monomania with delirium, this author relates the following history. "M. N. thirty-eight years of age, of a tall stature, yellow complexion, and spare habit of body, had been a gunner; he was then a journeyman. He had been subject to nasal hemorrhages, which had ceased for some months past. From that time forward, he became melancholy, and ceased to work. N. imagined that he was accused of having committed some crime; and in a fit of despondency, endeavoured to hang himself. He was bled in the arm and foot, and after other attempts at suicide, he was sent to Charenton. At first there was agitated manner, which disappeared in a few days; the patient continued sad and silent, with an uneasy look. He was put in the hall appropriated for those who had attempted suicide, in order that he might be the better watched. On the following day, and without any provocation, he struck the person next to him several times with the chamber utensil, threw himself on him and wished to kill him, in which he would have succeeded but for prompt interference. The patient was interrogated, and replied calmly that he heard his two brothers tell him to kill his neighbour, who wished to do him an injury. The next day N. seemed not to remember what he had done the evening before; he remained tranquil, sad and silentious after this event, as he had been before."

The forgetfulness of this act of violence on the following day, merits, says M. Georget, to be noticed, since it is an exception to the great majority of cases of mental alienation: hence M. Esquirol says simply that this madman *seemed* not to remember the act he had committed.

"There was," continues M. E. "some years ago, at the Salpétriere, a woman from the country, who wished to die, but who not having resolution to destroy herself, often said, 'I must kill somebody one of these days, that I may be made to die myself.' She had often attempted the life of her mother. When in the hospital, she endeavoured to kill one of the attendants, though this propensity was restrained by fear. She exclaimed frequently during the day, 'end my days, or else I will kill somebody.' She was much emaciated, did not eat, was sad, and had a fixed look."

All those monomaniacs, though acting under an erroneous idea and an error of judgment, have a known and avowed motive; they obey an impulse indicating reflection, and *even evince premeditation*. A very small number of them, after the commission of the fatal deed, *have sought to fly and to conceal themselves—all seem to have been conscious that they had committed a bad act*, except those who yielded to a religious hallucination. A close inspection of these persons enabled us to discover some corporeal derangement; sometimes there is evident disturbance of the nutritive functions.

Homicidical Monomania without Delirium. "We are unable (it is Dr E. who speaks) in this kind of insanity, to discover any intellectual disorder; the destroyer is carried away by an irresistible power, an influence which he cannot conquer, a blind impulse, a determination without reflection. We cannot conceive what prompts him without interest, motives, or intellectual bewilderment to a deed so atrocious and so contrary to the laws of nature."

In reply to the objections of those who consider this kind of monomania as a fiction, Dr E. asks, "whether the will may not, like the understanding and the affections, undergo vicissitudes according to numerous circumstances in life? Have the child and the old man the same power of volition as the adult? Does not sickness enfeeble it? Do not the passions diminish or exalt it? If things be thus, why may not volition be subjected to disorders, perturbations, diseased conditions, however incomprehensible the change may be to us." In confirmation of these views he gives the following among other cases.

"Madame ——, aged 36 years, of a strong constitution, unequal disposition, an excellent daughter and tender mother, enjoyed until the fourteenth year of her age excellent health, at least to all appearances. She had a fulness of frame, though she had not yet menstruated. All the signs of puberty were well marked: at each menstrual epoch, or rather every moment, the young lady complained of headach; her eyes were red; she was restless, irascible, moody; her face would soon be as injected as her eyes; every thing was disagreeable to her, and became a cause of irritation; she sought a quarrel especially with her mother, and finally yielded to the most violent anger. In this state her mother was always the object of her passionate bursts, of her abuse, threats and maledictions. Sometimes she attempted suicide; on two or three occasions she laid hold of a knife, and once I withheld her thus armed, from rushing on her mother. When the paroxysm was at its height, blood escaped by the mouth and nose, and sometimes by the eyes; then followed tears, a general tremor, cold extremities, convulsive pain of all the limbs, succeeded by a long swoon. The state of suffering was of several hours' duration. During the first part of the paroxysm this young lady would roll on the floor, strike her head against the wall or the furniture; give herself violent blows with her clenched hand and scratch her face. Her physiognomy, generally very mild, became at such times hideous; the colour of her face, ears and neck was of a violet red. The head was burning; the extremities very cold.

"From the onset of the fit which lasted one or two days, the symptoms became more and more aggravated, until they reached their acmé. The countenance was at first moody, the complexion fresh, the disposition unequal, exacting, quarrelsome; a gesture, a look, a refusal was the cause of great discontent, and the slightest incident was speedily the occasion of a lively irritation of violent chagrin; finally her anger broke forth. Sometimes the symptoms would be obviated by attention, desire to please, by the arrival of a stranger, or the presence of an uncle who was much beloved. Frequently also the fit would go off in disagreeable and unjust complaints against the whole household. She would be particularly irritated against her mother, or a younger sister. Sometimes she would create occasions for dispute, in order to hasten the progress of the fit, and reach the period of anger. She did not suffer in this last state, whereas in that preceding it, she experienced the most atrocious pains, especially in her head.

"The paroxysm once over, Mademoiselle —— was dutiful to her mother, asked her pardon, and was prodigal of every mark of tenderness to her. I have frequently advised and urged her to overcome the first symptoms of the fit, and represented to her how reprehensible and dangerous her conduct was. She would then weep, and exclaim with bitterness, *Why have I been made thus? I wish I were dead; how miserable I am! I cannot restrain myself when I have reached my angry time: I no longer see any thing, nor know what I do, nor what I say.* She had not a remembrance of all the circumstances of her paroxysms, and denied, with a look of surprise and regret, the particulars related to her. At sixteen years of age, the fits of anger were often replaced by hysterical convulsions. The disease progressively diminished and ceased at her seventeenth year, when the menses appeared, though in small quantity. After marriage the nervous symptoms disappeared, though she menstruated irregularly and in insufficient quantity. On no occasion was there the slightest trace of intellectual lesion.

"This lady is an excellent mother and very good daughter; but she retains a disposition at times unequal and a tendency to melancholy."

Our limits will not allow of our introducing at this time additional cases, or of giving some very judicious reflections on homicidical monomania by Dr ESQUIROL. Enough has been said to direct the attention of our readers to a theme so much abounding in interest to the medical jurist and moral philosopher. We shall return to it at a convenient season, recommending, in the mean time, all who have it in their power to peruse attentively the work of PINEL *sur l'Aliénation Mentale*, and that of GEORGET *sur la Folie*.

VI. SURGERY.

37. *High Operation.*—M. SOUBERBIELLE presented to the Academy of Medicine the case of an octogenarian, on whom he had the same day performed the high operation, and removed twenty-one calculi, two of which were encysted. The patient bore the operation very well, and in this case as well as in several others, the urine escaped by the urethra and not by the wound.—*Rev. Med. Jan.* 1828.

Vide former volumes of this Journal for cases of this operation.

38. *Compound Luxation of the Elbow Joint.*—Prof. STEVENS of New York has recorded a case of this accident, successfully treated without amputation. —*New York Med. & Phys. Jour. No.* 25, *N. S. No.* 1.

The extremities of the ulna and radius were thrown behind the condyles of the humerus; the integuments in front were lacerated transversely to the extent of two and a half inches;—the tendons of the biceps and of the brachialis internus rent asunder;—the nerves stretched before the condyles of the humerus, and the brachial artery was torn and projecting from the wound. The hemorrhage was considerable: the lower end of the artery was not visible, but the upper extremity could be seen and was still pulsating. The orifice was contracted to the size of a crow quill, but not pervious. A ligature was applied to the torn artery at a point where its vascular communications remained. About an inch of the lacerated extremity was removed, and found to be filled with a coagulum.

The forearm was placed at right angles to the arm after reduction. No particular difficulty occurred; the pulse in the radial artery was felt on the third day after the accident, and a counter-opening became necessary for the discharge of pus at the lower part of the joint. The patient was discharged well in eight weeks, having a very slight flexibility at the elbow, and perfect command of the hand and fingers.

39. *Amputation at the Elbow Joint.*—Dr J. KEARNEY RODGERS, Surgeon to the New York Hospital, amputated at the elbow joint in November last, for a severe gun shot wound of the forearm. No fever and no unusual symptoms supervened; the wound was found united in two days, except in the course of the ligatures. In three weeks, the ligatures came away, leaving the stump sound and more useful than when the ordinary place for amputation is selected.—*Ibid.*

40. *Imperforate Vagina.*—M. VILLAUME of Metz reported to the French Academy of Surgery the case of a young female, whose life was in great danger from a gradual accumulation of the menstrual fluid in the uterus in consequence of an imperforate vagina. Her illness commenced at fourteen years of age; and at sixteen years old, the abdomen was of the size of a female advanced in pregnancy. The external organs were natural; the hymen even existed, but a dense cellular mass occupied the situation of the vagina. A cautious dissection was made between the urethra and rectum, avoiding each of these canals, to the body of the uterus, which was then punctured. A bloody discharge commenced, and continued for several days to the relief of the patient. The canal became fistulous and gave issue to the menses, and, during the interval of the menstrual periods, to an abundant leucorrhœa.—*Nouv. Bibliotheq. Jan.*

41. *Ligature beyond the Tumour; Mrs Denmark's Case.*—Vide last number

of this Journal, p. 466, for the continuation of the interesting case of Mrs D. whose right subclavian artery was tied by Mr **Wardrop** for an aneurism of the arteria innominata, and of whose subsequent condition such opposite accounts have been given. Dr **Johnson** reports the patient alive on the 14th of January. On the 5th the aneurismal tumour was again taking an outward direction—the clavicle was again rising and becoming dislocated, and in short the poor patient was nearly in the same pathological condition as before tying the artery beyond the aneurism. January 14th, clavicle still rising, breathing very bad, though not so much so as at one or two periods within the last weeks, and no pulsation could be felt in the right radial artery since the 27th of December.

We have not seen any account from Mr **Wardrop** later than the 5th of December. In our next number, the result of the case will probably be given.

42. *Traumatic Tetanus.*—In the peninsular war, Dr **Barry** tried, with success, in tetanus from wounds, alternate doses of carbonate of soda and opium. **Hufeland** has reported a case illustrating this practice—copied by the Revue Medicale and also by the Medico-Chir. Review for April, from which we condense the following account. A healthy young woman was seized with tetanic symptoms, a month after receiving a contused wound on the instep of the left foot and before the wound had healed. The ulcer was in a bad condition.

Thirty-two ounces of blood were abstracted, purgatives given and a poultice applied to the ulcer. Bowels being opened, "*the method of* **Stultz**" (so this practice is termed) was adopted, and in the course of twelve days, 224 grains of opium were given in alternate doses with carbonate of potash: the quantity of the latter is not mentioned. The spasms became less severe and frequent, till a general swelling of the body came on, when they ceased altogether. The swelling seemed to be anasarcous, and disappeared under the use of diuretics, but was followed by an eruption on the skin resembling scarlatina. The patient eventually recovered.

43. *Inguinal Aneurism—External Iliac tied.*—The external iliac artery was secured on the 21st of February 1828 by Mr **Brodie**, for a large aneurism of the femoral artery, apparently extending the whole length of the artery. Pressure on the external iliac, before the operation, arrested the pulsation; whilst pressure on the superficial femoral below the aneurism lessened the pulsation, but had little or no effect on the tumour. When the silk ligature was tied, the pulsation ceased, and did not return; but the size of the tumour was not much reduced at the time: since, it has steadily decreased. The ligature had not come away at the last date (March 8th).

Mr **Brodie** operated on **Abernethy**'s plan in preference to sir **Astley Cooper**'s for several reasons: he had twice adopted the former operation with success, and therefore it was more easy to him. There was reason to believe the lower portion of the artery was diseased; hence the higher the ligature is applied, the safer. In sir A. **Cooper**'s operation there is danger of wounding the epigastric artery; an accident which happened in 1821 to M. **Dupuytren**, the patient dying of peritoneal inflammation.

Another objection to **Cooper**'s operation is that the ligature is applied just above the giving off of the epigastric and circumflex ilii, and hence the internal coagulum is short, and secondary hemorrhage more probable. A patient of **Dupuytren**'s was greatly exhausted by a secondary hemorrhage from the lower portion of the artery, and on examination, the epigastric artery was found enormously enlarged and to have brought the blood into the femoral, almost to the verge of the ligature, on the separation of which, rupture and hemorrhage ensued. In another case, sir A. **Cooper** secured the artery *immediately below* the origin of the epigastric and circumflex arteries, and secondary hemorrhage followed. The same thing happened to Mr **Cline**, and lately at St George's Hospital dangerous hemorrhage followed the coming away of a ligature from the femoral artery a little below the giving off of the profunda.—*Medico-Chirurg. Rev. April.*

44. *Exuberant Callus, dissipated by means of a Seton.*—This case is reported by professor C. A. WEINHOLD of Halle. John X. R. aged eighteen years, fractured the os femoris near its middle, and was treated by a neighbouring surgeon in the ordinary way, to the end of the fourth week. He then returned to his work in the country. At the expiration of ten weeks, the limb was shortened two inches, and the callus augmented to the size of a child's head. He came under the care of Dr WEINHOLD. The callus was eighteen and a half inches in circumference; the cellular tissue, above and below the tumour, was loaded with lymph, and abscesses and fistulæ had formed. Extension and counter-extension were tried for eight days; but as three months had elapsed since the accident, no mechanical measures would prove effectual. Determining not to abandon his patient, Dr W. decided on perforating the callus, and to carry through it a seton imbued with stimulating substances, to excite inflammation and suppuration of the bone, and finally the softening and absorption of the callus, that he might restore the bone to its original length by regular and judicious extension. On the 11th of November (year not given), Dr W. prepared a sharp-pointed drill, mounted on a whimble, and pierced the soft parts with the point of the instrument, which was then gently turned until he perforated the external layers of the callus: he then traversed, suddenly, a cavity, four inches in depth before reaching the opposite side. This was then perforated and finally the muscles and skin, and the seton was introduced: not more than an ounce of blood was lost. For three days, the part was covered with cold cataplasms. On the 4th day, the seton was imbued with the balsam of Arceus, and was drawn across the wound twice a day. Suppuration in the cellular tissue was very abundant during the 5th week, the pus being discharged at openings above and below the callus. These fistulæ were cured by pressure, a short time after the induration of the cellular tissue had been dissipated. At the end of the 6th week, the callus was very painful, and its temperature augmented, which symptoms were dissipated by cold poultices. In the 7th week, the long desired suppuration of the callus occurred, and on pressure with the finger, a large opening was felt which indicated that the favourable time had come for employing extension. This was persevered in, and during the 10th week, the limb became so much lengthened as to be only two lines shorter than its fellow. The seton was continued to the 12th week; it was then removed and the wound cicatrized. Some weeks after, the callus was considerably diminished, the patient walked without crutches, and the affected thigh presented nearly the same size and appearance as the sound one. *Journal des Progrès, Tome VIII.*

45. *Case of Contraction of the Canal of the Larynx relieved.*—Elizabeth Oswald, æt. 27, in attempting suicide in September 1826, wounded the crico-thyroid ligament. She recovered with the loss of voice and with a fistula of the larynx through which she breathed. On the 15th of April 1827, Mr LISTON, having examined the neck and discovered a very minute orifice leading towards the glottis, began to introduce bougies, about the size of darning needles, into the part of the larynx above the wound; by gradually increasing the size of the bougies, the passage was brought about the end of June to its natural diameter. About the middle of August, the lower part of the trachea was dilated by a tube, gradually increased in size, so that one of sufficient diameter might be passed by the mouth. Three weeks afterwards, the short tube was removed, and a long one was introduced by the wound up into the mouth, there laid hold of, and pushed down into the trachea. Severe coughing for half an hour was thus excited. This tube, nine inches long and equal in diameter to the largest æsophagus tubes, was retained in the windpipe for fifteen days, during which it excited great salivation, loosened the teeth and extremely reduced the patient's strength.

To close the wound, the cicatrix was, on the fifth day after introducing the tube, dissected out by two elliptical incisions, and the edges of the wound united by suture. The tube being removed on the 15th day, she breathed well. In the morning, however, her breathing became very difficult, and *tracheotomy* was performed. A silver tube was introduced and retained for five days; but was then replaced by a smaller one.

The actual cautery was applied to the edges of a small sinus which remained on October 17th; and on the 26th it was reapplied to the opening, which had nearly closed. On the 28th the tube was removed; and a few days after the wound was completely closed. The woman now breathes with comparative ease through the larynx, and is slowly recovering the use of her voice.—*Ed. Med. and Surg. Jour. Jan.* 1828.

46. *Traumatic Tetanus.*—In the Nouvelle Bibliothéque for March, three interesting cases of traumatic tetanus are given, in which cold affusions appeared to be instrumental in saving the lives of the patients. In each, there was a horror of this remedy, and in each the affusions were repeated, until a state analogous to syncope was induced. The patients were then put to bed, and carefully rubbed with warm flannels, while warm wine and paregoric were administered. Reaction was thus induced with great relief to the spasms. The affusions were to be repeated, and the disease was completely subdued in eight days. Other treatment, such as with mercury, camphor, laudanum, cathartics, &c. was employed, but evidently with no advantage, excepting by facilitating reaction and calming the mental agitation of the patient. The cases were presented to the Atheneum of Medicine at Paris, by Dr DOUCET, and occurred in different parts of this country (America). The first was a man of a robust constitution, 37 years of age, with traumatic tetanus from a wound of the big toe, which had completely cicatrized on the 24th June 1823. On the 26th, the tetanic symptoms were well marked. On the 28th, cold affusions were employed; after opium, calomel, and injections had been ineffectual. They were repeated occasionally as the patient's consent could be gained, and he was quite well July 12th.

The second case was also a male of good constitution, 22 years of age, affected with the disease at Montreal, July 1806. Cold affusions were employed and he was cured in 13 days.

The third case, a robust man, 34 years of age, was cured at the end of eighteen days.

In none of these cases were the cold affusions resorted to as frequently as was desirable, owing to the repugnance of the patients; but they always appeared to be advantageous, and of course would have effected a cure sooner if they had been more diligently employed. The following authorities are quoted by the author as sanctioning the practice: CULLEN in his Practice of Medicine; M. FOURNIER PASCAY, Dictionnaire des Sciences Medicales; Dr HARRIS of Pennsylvania, New Medical Repository, Vol. 3; Dr ARCHER of Maryland, Id. Vol. 9; Dr PRIOLEAU of South Carolina, Amer. Med. and Philosophical Register, Vol. 3: Dr WRIGHT of Jamaica, Dr DOVER at Cayenne, and Dr RUSH of Philadelphia, are cited by Dr VALENTINE in Jour. General de Med. Vol. 40, as having used cold affusions with advantage. So also several English physicians, as HUNTER, CURRIE, and DARLYNG, vide Thomas's Practice, and Edinburgh Journal, 1807. In a work published at Paris, entitled *Precis sur le Tétanos des Adultes*, HEURTELOUP recommends the cold bath; and that the individual, on leaving the water, should be placed in a bed well warmed, and take a glass of wine, or ten or twelve drops of the aq. ammoniæ, in a few spoonsful of water. He cites a case exhibiting the good effects of this treatment.

47. *Amputation of the Lower Jaw.*—This operation has now been so frequently executed with success, that surgeons are directing their attention to the best means of preventing, as much as possible, the subsequent deformity. The enterprising and active surgeon, M. LISFRANC, in November last, extirpated the left portion of the lower jaw from the chin to the ramus, for a sarcomatous tumour involving the bone, extending under the tongue, and contaminating, in a slight degree, the lymphatic glands. To prevent deformity, M. LISFRANC, instead of making the ordinary transverse incision across the cheek, made a perpendicular

cut from the middle of the free border of the lip to the base of the jaw, dividing all the soft parts to the bone. The incision was continued an inch further in the same direction; it was then made transversely from before backwards to a point one quarter of an inch below and before the angle of the jaw, and extending only through the skin and cellular tissue. The flap thus formed was dissected upwards, and the bone and diseased mass extirpated in the usual manner, without any troublesome hemorrhage. The fluids from the mouth and wound had a free outlet in a depending position; part of the wound healed by the first intention, part by granulation; and a fistula, kept up by the discharge of saliva, &c. was formed on the side of the neck. M. Lisfranc succeeded in curing it, by placing in the mouth, at a point corresponding to the external orifice, a small piece of sponge which absorbed the fluids that retarded the cure. The sinus then healed in four days. Little deformity resulted; the cicatrix being nearly covered by a cravat. The left side of the jaw is but slightly depressed; while the right side has deviated a little to the left, but can easily be replaced in its ordinary position. The left ramus of the bone is carried a little inwards. The patient speaks with much distinctness and can take solid food.—*Revue Med. March*, 1828.

48. *Artificial Anus cured.*—Professor Graefe, of Berlin, has successfully managed an artificial anus resulting from a sloughing inguinal hernia. Uninterrupted dorsal decubitus, cleanliness, occasional closure of the orifice by pledgets of lint, liquid aliment, and the daily administration of two laxative enemata, were so effectual, that in six weeks, the flow from the fistula ceased and the external orifice was closed by pressure. In two months and a half, the patient was perfectly well.—*Revue Med. March.*

49. *Cancers of the Skin.*—M. Graefe has successfully employed, in cancers of the skin, a mixture of the perchloride of mercury (corrosive sublimate) with the mucilage of gum arabic. This liniment forms an application much more easy, and more efficacious than all the usual pomatums, powders, &c. It may be allowed to dry on linen or pieces of lint, and thus be introduced into narrow fistulæ, and be applied to the smallest spot without fear of any injurious consequences.—*Revue Med. March.*

50. *Traumatic Epilepsy cured by Trephining.*—We have been favoured by the editors, with the first number of the Transylvania Journal of Medicine, and have been much pleased with the contents of this new western periodical.

The first paper is from the distinguished western surgeon professor Dudley, in which are many valuable cases and observations on injuries of the head. Our limits will allow of but a short summary of a few cases of epilepsy following blows on the head, relieved or cured by trephining; and we regret that we cannot offer some of the author's deductions, which, although not original, are opposed to some leading opinions of the day.

In December 1817, Mr K. suffered pain in the upper part of his head. This continued; and in September 1818, he consulted Dr. D. About Christmas epileptic convulsions came on, and on the 16th of April 1819, he was trephined where the bone appeared to be depressed, and where several tumours had formed. The pericranium was thickened and very sensible; the bone porous, and very firmly adherent to the dura mater over the longitudinal sinus. The dura mater was found separated from the brain by a fluid deposition, so that the pulsation of the brain was not evident. On the fifth day, suppuration was free; the fluid under the dura mater was absorbed, so that the brain had regained its proper level and pulsated with unusual vigour. A mercurial course was prescribed. Two slight attacks of epilepsy supervened, but in three months he was dismissed well. Subsequent observation induced Dr D. to regard the cure as radical.

W. T. when five years old had a blow on the parietal bone, followed by concussion, apoplexy, and paralysis. In two months he recovered his mental and corporeal powers. He became subject however to severe cephalalgia and was weak and delicate. At fourteen years of age he was seized with convulsions. His general health improved, but his mind failed, and he was removed from school. In the spring of 1825, Dr D. was consulted, the patient being then 21 years of age, with impaired memory, judgment, &c. On the 10th of May, the parietal bone was trephined where a depression existed. A spiculum of bone, about one inch in length, was found projecting from the inner surface of the cranium. It penetrated the dura mater, and communicated with a large preternatural sinus, "from which issued a stream of blood as thick as a man's little finger, which continued to flow until it was judged proper to check it by means of pressure." The dura mater was diseased and of a dark colour. A convulsion supervened, during which "a stream of blood issued through the opening in the dura mater that projected three or four feet." Two more fits followed with like discharges of blood. For three days and nights large quantities of serous fluid were evacuated from the wound. 4th day. Dressings were dry, suppuration followed. In 30 days, the wound was healed. No convulsion came on after the operation. His mental and bodily faculties recovered their activity, and in July, six weeks after the operation, he returned home in perfect health. With the exception of one fit, excited by dancing and by excess in eating and drinking, he has remained well.

Gofourth, when 5 years old, had an injury of the parietal bone: suffered few bad symptoms until 15 years of age, when epilepsy supervened. The paroxysms returned every three or four weeks, and at the age of 23 he was imbecile in body and mind. In February 1826 Dr D. trephined him on the parietal bone. Much serum escaped during the operation. The dura mater was found deficient to the extent of a twelve and a half cent piece of silver, which exposed a sinus extending to the petrous portion of the temporal bone. A spinous process projected about half an inch from the internal surface of the bone. The sinus in which the serum was collected was large enough to receive a hen's egg. On the second and third days two slight attacks of epilepsy. On the 4th suppuration took place. Improvement of mind and body was daily manifested and at the end of the fourth week, the wound was cicatrized and the patient returned home quite well. During the summer he was employed as a fireman on board of a steam boat on the Mississippi, and in August reported himself as quite well.

In two other cases, the operation was performed with much advantage; but the recovery was not quite complete, perhaps owing to subsequent neglect. One, O'Brien, received in 1822 a blow on his head, by which he was deprived of motion and intellect for several weeks. He afterwards suffered from severe convulsions and almost constant pain. He spoke with difficulty—senses of taste and smell very nearly destroyed—all his muscles seemed preternaturally contracted. The operation was performed: dura mater very vascular; no other apparent alteration about the wound: but towards evening patient said his limbs were "unlocked," and there was a perfect relief to the organs of speech. On the third day suppuration ensued, and the pain and other symptoms disappeared; but on the sixth day, a convulsion supervened. His appetite became ravenous, which he often indulged. His general health improved. He was able to walk out; his mind was not perfectly correct; he had some slight convulsions; withdrew from his medical attendant and has not since been heard of.

Froman consulted Dr D. in April 1827, having fifteen years previously had a fracture of the superior and posterior portion of the parietal bone. His memory was immediately impaired; in two years convulsions supervened and finally fatuity. The operation was performed: bone and dura mater healthy; no pulsation in the brain, owing evidently to effusion under the dura mater. On the fifth day suppuration was established; and the brain was now found at its proper level, the fluid having been absorbed. For two weeks, all was flattering. The convulsions however returned and continued; but were reported to be less frequent and more mild than previous to the operation.

These cases with others on record are certainly very encouraging, and show, as Dr D. observes, that the operation of perforating the bone is not in itself very dangerous, especially when the patient has the advantage of a good constitution and a pure atmosphere.

We would recommend the whole of Dr DUDLEY's paper to attentive perusal, although we believe he carries some of his rules rather to an extreme. In our first volume, p. 183, will be found a notice of similar cases from sir A. Cooper; and in our second vol. p. 204, the case in which Dr Rogers of New York successfully operated under similar circumstances.

VII. MIDWIFERY.

51. *Cæsarian Operation.*—A young woman of a frail, rachitic temperament, was admitted into the hospital at Florence in Italy, on the 13th May 1827, at 10 o'clock, A. M. She had been in labour for sixty hours at the full period of utero-gestation; pains unfrequent, os tincæ dilated, the membranes ruptured, the umbilical cord protruding at the vulva without pulsation, and the fœtus without motion for six hours. The antero-posterior diameter of the superior strait was so much diminished, that the physicians decided on the necessity of the Cæsarian section. This was executed in the usual manner and a full grown but dead fœtus was extracted. A pledget was interposed at the inferior portion of the wound; the remainder was closed by fine sutures.

Evening. Fever; was bled to ʒxii, and during the night again bled to ℥ix.

2d day. Fever, vomiting, cephalalgia, ischury. Catheterism, two bleedings each to ʒviii, enema, diluents.

3d day. Symptoms continued. A fifth bleeding, catheterism, injections.

4th day. Better. Constipation remains. Ol. Ricini, ʒss, produced abundant liquid and yellow stools.

5th day. Return of fever, abdomen tympanitic, and painful on pressure, lochiæ suppressed, cephalalgia, dryness of pharynx. A sixth bleeding, to ʒviii, relieved. Suppuration at the wound.

6th day, and early on the 7th. Amelioration of symptoms—return of lochiæ, lancinating pains in the mammæ. *At night.* High fever; was bled twice, once ʒiv, the second ʒii.

8th day. Better. Wound suppurating at inferior part; the rest united.

Patient continued to improve, and on the 30th day left the hospital quite well. —*Annali Universali, Milano.*—*Revue Med. Jan.* 1828.

52. *Arm and Shoulder Presentations.* Dr Robert Lee read a short paper before the Westminster Medical Society on the 5th of January, recommending the sacrifice of the child in cases of arm and shoulder presentations where turning was unadvisable or impracticable. When we have proof that the child is dead,—that the pelvis is too small to allow a living child to pass—or that there is great improbability of bringing a living child into the world, even by turning, we are advised to disjoint the arm, at the shoulder, introduce a blunt hook, so as to take hold of the spine as low down as possible,—and bring away the fœtus double—thus imitating the operations of nature, in some cases of spontaneous evolution.—*Medico-Chirurg. Rev. April.*

53. *Birth of Five Children at one Accouchement.*—Dr Weiss records the case of a female aged 27 years, married for five years, and having two years previously given birth to twins, being brought to bed of five children; two boys in succession, then one girl, afterwards a third boy, and finally, a girl. Each child had its own membranes and placenta. They were all regularly formed, but not full grown, and had the appearance of old age. They slept continually and their temperature was inferior to that of new-born infants. None of them lived beyond the third day. The mother did well.—*Arch. General. March.*

54. *Retention of the Placenta.*—Dr And. Primus relates the case of a female, confined with a dead infant on the 25th of January. The midwife could not succeed in bringing away the placenta, and in her attempts ruptured the cord. The placenta remained,—no contractions and no hemorrhage supervened; never-

theless the woman enjoyed perfect health until the 14th of May following, when some hemorrhage and colic pains came on. These re-appeared on the 22d of the same month with more severity, and were followed by the expulsion of the placenta.—*Archiv. General. May.*

55. *Diseases of the Placenta.*—M. Brachet believes that the placenta is liable to the same diseases as other organs, and that they are excited by similar causes; that of these affections inflammation is the most common, terminating by schirrus, suppuration and organic adhesion; that the different kinds of concretions, whether osseous, calcareous, hydatids, &c. exist independently of inflammation; and finally, that the diagnosis of these diseases is very obscure, and often cannot be determined. Inflammation can sometimes be detected and may be treated on rational principles.—*Jour. General, Jan.* 1828.

A case has lately occurred to one of the editors of this Journal, which may be referred to inflammation. A female, the mother of several children, was delivered, in June 1827, of what she deemed a false conception. Her menses returned three or four times at the regular periods, and she again had the usual indications of incipient pregnancy. She, however, never quickened; suffered more than usual uneasiness; complained much of pain in her back, hips and thighs, and often of a burning sensation in the region of the uterus. She went to what she considered the eighth month of gestation, and was delivered, after a short but severe labour, of what at first view, appeared a mere fleshy mass. On examination, this mass was found to consist of a fœtus and its appendages. The fœtus was about three months growth, of a chocolate colour, surrounded by ℥iii. of a chocolate coloured fluid, and enveloped in its membranes, having the same dull brown colour. The smell was unpleasant and the membranes tore readily. The placenta formed the largest portion of the mass, was much indurated, and had the fresh colour of tumours lately separated from the living body. It would appear that the child died about the third or fourth month of gestation; but that the placenta continued to live to the eighth month, and was affected, before or after the death of the fœtus, with inflammation. Does not this case confirm the opinion, that the organic life of the placenta, both fœtal and uterine portions, depends on the parent, and not in any measure on the child?

56. *Puerperal Peritonitis cured by Ice.*—A female was seized with severe symptoms of peritonitis on the fourth day after parturition. She was bled to ℥xvi; ice water for drink, and ice to the abdomen. In three hours, the severity of symptoms was diminished; thirst and vomiting had disappeared; abdomen less tumid, and pressure less painful. In the evening, amelioration still greater; the belly quite soft; a natural stool occurred, and the patient had some sleep. The application of ice was alone continued, and next day perspiration came on. The mammæ filled with milk; the lochiæ soon appeared, and in four days no symptom of peritonitis remained.—*Archiv. General. for Jan. from the Jour. of the Roy. Soc. of Med. at Toulouse.*

VIII. CHEMISTRY AND PHARMACY.

57. *Bromine, an Elementary Body.*—Facts of a convincing nature, having been presented to the French Academy of Medicine, going to show the elementary nature of bromine, that learned body decided, that the assertion of M. Dumas, that bromine is a compound of chlorine and iodine, should be considered as retracted, and ordered that an entry to that effect should be made upon the minutes.

58. *Discrimination of Bromine from Iodine.*—M. Pomier, an apothecary of Salies, Lower Pyrenees, conceives that from the numerous analogies between

bromine and iodine, these bodies are liable to be often confounded. He states that starch becomes of an intense crimson colour by the action of bromine; while, as is well known, it strikes a blue colour with iodine.—*Nouvelle Bibliothèque Médicale, Dec.* 1827.

59. *Discrimination of Baryta and Strontia from one another.*—MM. JULIA-FONTENELLE and QUESNEVILLE communicated to the French Academy, on the 11th of Feb. 1828, a very simple method of distinguishing baryta from strontia. If, upon the addition of a few drops of sulphuric acid, the doubtful earth becomes red hot, it is strontia.

60. *New Variety of Borax.*—In the Journal de Chimie Médicale, for April last, we find an interesting memoir on borax, by M. PAYEN.

In his manufactory for purifying borax, the author observed an octohedral variety, harder and heavier than the ordinary prismatic salt. Upon making a comparative analysis of the two kinds, he considers the prismatic variety to consist of

2 atoms	Boracic Acid,	70.4
1	Soda	31.27
10	Water	90

and the octohedral variety of

2 atoms	Boracic Acid	70.4
1	Soda	31.27
5	Water	45

The above are deduced proportionably from M. PAYEN's numbers, by deducting one-fifth from each, so as to make them conform with the hydrogen unit, which is most familiarly known in the United States. It is seen above, that the ratio of BERZELIUS's number for soda (adopted in France) to that for oxygen, is not the same as that admitted in England and America. According to the English chemists, it is 32. Disregarding the water, and deducing the proportion of the boracic acid on the assumption that soda is represented by 32, then we have M. PAYEN's experimental proportions expressed thus:—

Boracic Acid	72.04
Soda	32

Now Dr THOMSON (First Princ. Chem. II. 270) gives these proportions:

2 atoms	Boracic Acid	48
1	Soda	32

The discrepancy between these analyses is very great; but they may be reconciled in the following manner. It is well known that the borax of commerce is a salt in which the alkaline property prevails, and hence, it is generally called subborate of soda. This is the salt analysed by Dr THOMSON. Now, M. PAYEN conducted his analysis by *saturating* a pure boracic acid with carbonate of soda. May it not have happened that the experimenter obtained a compound, containing a larger proportion of acid than exists in ordinary borax; and that the difference between his analysis and Dr THOMSON's may depend on this circumstance? Whether this conjecture be worthy of any attention or not, it is at least certain, that assuming the quantity of boracic acid obtained by Dr THOMSON to be two atoms, the quantity found by M. PAYEN corresponds with remarkable precision to three atoms; for three times 24 make 72.

We would, therefore, suggest, that M. PAYEN is probably wrong in considering the quantity of boracic acid which he obtained as corresponding to two atoms.

61. *Artificial Ultramarine.*—GAY-LUSSAC announced, at a meeting of the French Institute, held on the 4th of Feb. last, that M. GUIMET has discovered a method of forming ultramarine of every variety, by availing himself of the data, furnished by the analysis of the lapis lazuli, performed by MM. DESORMES and CLEMENT. The artificial product has a richer tint, and is more brilliant than the natural ultramarine. The Society of Encouragement had offered a prize for this

discovery, of which the committee of the chemical arts had foreseen the possibility.

62. *Analysis of the Water of the Dead Sea.*—In the Journal de Chimie Médicale, there is given the following analysis of the water of the Dead Sea, by Ch. Gmelin.

Chloride of calcium,	3.22
Chloride of magnesium,	11.77
Bromide of magnesia, [?]	.44
Chloride of sodium,	7.08
Chloride of potassium,	1.67
Chloride of aluminum,	.09
Chloride of manganese,	.21
Muriate of ammonia,	.01
Sulphate of lime,	.05
	24.54
Water,	75.46
	100.00

This analysis is interesting, as embracing the newly discovered element bromine.

63. *Analysis of Human Milk by* M. Payen.—Seven hundred and seventy-five grains of milk, taken in the seventh month of lactation from a healthy woman, contained

Water	666.5
Fatty matter	39.9
Caseum, and traces of insoluble salt	1.4
Sugar, soluble salts and traces of an azotized matter	59.0
	766.8

This analysis gives 13 per cent for the solid matter contained in human milk.

Two additional analyses of human milk, by M. Payen, give nearly the same result; the solid matter, varying from 13 to 14 per cent.

Compared with goat's milk, woman's milk differed in possessing a decided alkalinity, and in containing about half as much caseum.—*Journal de Chimie Médicale, Mars* 1828.

64. *Reduction of Silver from the Fused Nitrate.*—A. Chevallier has given a note on this subject, in the Journal de Chimie Médicale for 1827. It is well known, that copper precipitates the silver from a solution of nitrate of silver. But M. Chevallier has observed the novel fact, that the same decomposition takes place in the dry way. Thus a stick of lunar caustic, being placed on one of its extremities on a plate of copper, was totally decomposed, without losing its shape. M. Chevallier was led to make this observation, in consequence of being consulted by a physician, on the cause of the inactivity of lunar caustic, held in a port-caustic made of copper.

65. *Action between Nitrate of Silver and Linseed.*—Apothecaries in France are in the habit of keeping the fused nitrate of silver in linseed. It would seem, however, that this is an improper practice; for M. Dulong has proved, in a memoir communicated by M. Henry to the Section of Pharmacy of the French Academy, that these substances act upon each other, even without the aid of moisture. When the substances are dry, the mutual action is indeed slow; but nevertheless, an appreciable quantity of the fused nitrate is absorbed by the organic matter of the seeds, whereby minute excavations are formed on the surface of the cylinders of the nitrate.

Others seeds have the same effect. M. **Dulong** has observed certain coloured rings on some grains of linseed, which had been touched with the fused nitrate, and considers the appearance to be an electro-chemical phenomenon. It is very important, according to this chemist, to avoid employing, in pharmaceutical operations, metallic vessels fabricated of more than one metal.

On the communication of this memoir from M. **Dulong**, M. **Robiquet** took occasion to mention, that M. **Deyeux** had seen a case in which death was produced by the internal use of linseed, in which fused nitrate of silver had been kept.

66. *Method of Detecting Opium.*—In the Journal de Chimie Médicale for February 1828, M. **Orfila** gives a note on the method of Dr **Hare** (misspelt **Have**) for detecting minute quantities of opium, originally published in the Philadelphia Journal of the Medical Sciences. This method consists in precipitating a meconate of lead by acetate of lead, liberating the meconic acid by sulphuric acid, and then adding a solution of persulphate of iron, which will produce a characteristic red colour. The Parisian professor has repeated Dr **Hare's** process successfully; and remarks that it is preferable, without explaining why, to wash the meconate of lead on a filter, to decompose it by sulphuric acid, and filter anew to separate the sulphate of lead, and then to act on the persulphate of iron by the meconic acid, after passing the filter.

The examination of Dr **Hare's** method led M. **Orfila** to try another process, which he conceives to be more simple. He remarks, that as it is only necessary to have meconic acid in a free state for the persulphate of iron to strike the red colour, it must be clear that by acting directly on the meconate of morphia, existing in opium, by sulphuric acid, the meconic acid will be liberated, and, on the addition of a salt of iron, will undergo the characteristic change of colour. Now M. **Orfila** states this to be the fact; and further, that as the persulphate of iron always contains an excess of acid, the simple addition of the ferruginous salt is sufficient to redden the infusion of opium, laudanum, or the solution of the watery extract. M. **Orfila** subjoins to his remarks the following experiments.

1st. A grain and a half of **Sydenham's** laudanum (vinum opii), diluted with three ounces and a half of water, were mixed with some drops of persulphate of iron: the colour was changed from a light yellow to red. To appreciate the change of colour satisfactorily, it is best to examine the liquid comparatively with another portion, to which the ferruginous salt has not been added.

2d. With three grains of laudanum, and the same quantities of water and persulphate of iron, the red cloud was very distinct.

3d. The fourteenth of a grain of opium, diffused in four ounces of distilled water, and allowed to remain for ten minutes, produced, on the addition of a few drops of persulphate of iron, an evident red cloud. With the seventh of a grain of opium in the same quantity of water, and tested with the same amount of the persulphate, the red colour was very distinct.

4th. Similar results are obtained, when the fourteenth or eighth of a grain of the aqueous extract, dissolved in water, is used.

M. **Orfila** considers these experiments as proving,—1st. That the persulphate of iron is a very delicate test for meconic acid, contained in opium, the watery extract of this substance, and laudanum; 2d. That it is not necessary, as directed by Dr **Hare**, to treat these substances with the acetate of lead and sulphuric acid, for the purpose of developing the red colour on the addition of the persulphate of iron, but that it is sufficient to act directly on the opiate preparations with this salt; 3d. That Dr **Hare** is wrong in regarding the persulphate of iron as a means of detecting minute quantities of opium in solution; as this salt can be only considered as a reagent, no doubt very important, to be added to those heretofore employed, for ascertaining the presence of morphia [meconic acid ?]. It would be reprehensible in any one to affirm in a court of justice that a matter contained opium, *merely* because it was reddened by the persulphate of iron.

On the above observations, we have only one remark to make. It may be true that the method, proposed by Dr **Hare**, does not, strictly speaking, detect *opium*; as the agent which he employs acts on one constituent only of opium, and this constituent may possibly be present, either uncombined in a fluid, or in some unusual state of combination. The peculiar colour produced by the test, therefore, strictly speaking, only demonstrates the presence of meconic acid; while we have the violent presumption, that it is associated with opium in some form or other.

67. *Extraction of Morphia without the use of Alcohol.*—On the 9th February last, MM. **Henry**, Jun. and **Plisson** communicated to the Section of Pharmacy, a process for extracting morphia without the use of alcohol. It is founded on the fact of the facility with which morphia, mixed with narcotin, may be separated by the action of very weak muriatic acid. The liquor exhibits hardly a sensible acidity, when the morphia is held in solution as a muriate, while the narcotin is not sensibly dissolved. The following is the process:

Treat a pound of opium, at a gentle heat, with twice its weight of distilled water, acidulated by muriatic acid. Repeat this treatment three times, and after having concentrated the filtered or decanted liquors to two-thirds, add a slight excess of liquid ammonia of 12°, or of caustic hydrate of soda of 2° or 3°. Collect the precipitate, (which consists of resin, caoutchouc, narcotin, morphia, and a little extractive) wash it, and treat it repeatedly at a gentle heat with a very weak muriatic acid, so long as this acid takes up any thing, allowing it to be in slight excess. The yellow liquor obtained by the action of the acid, and which contains muriate of morphia, a little resin, and colouring matter, is then heated with animal charcoal, and evaporated to the point of crystallization. The crystals are to be twice purified by new crystallizations, and the mother waters to be concentrated and decomposed, as often as may be judged necessary. The crystals of muriate of morphia, obtained after three crystallizations, being dissolved in a small quantity of acidulated water, are decomposed by a slight excess of ammonia of 12°, and the morphia which precipitates is collected. In this state, it is sometimes slightly yellowish; but it is free from narcotin, and furnishes handsome crystals by means of alcohol and charcoal. When saturated with muriatic or sulphuric acid, it forms very white and well crystallized salts, which are much more readily prepared than the same salts, formed by saturating crystallized morphia, which is difficultly soluble in alcohol and the acids.

MM. **Henry**, Jun. and **Plisson**, having alluded to the remark of **Dublanc**, Jun. and **Pelletier**, in regard to the inconstant action of the acetate of morphia as usually prepared, it being in fact a variable mixture of real acetate and morphia,—propose to substitute for this salt the sulphate or muriate, both of which salts crystallize readily, and are easily identified.

68. M. **Girardin's** *Process for Morphia.*—This process was detailed to the Section of Pharmacy of the French Academy of Medicine, by M. **Henry**, Jun. It is as follows:—Exhaust opium by hot water, and reduce the liquors obtained to one-third. When cold, add an excess of ammonia. The precipitate produced is collected, dried, washed with weak alcohol, and treated with sulphuric ether. The residue is then dissolved in sulphuric acid, and the sulphate formed, decomposed by ammonia. M. **Girardin** states that he has obtained a greater product of morphia by this process than by any other.

On the subject of morphia, we refer our readers to Dr **Staples's** process, given in the last number of this Journal. It is some time since the writer was obligingly furnished by Dr **Staples** with portions of morphia, both pure and impure, as obtained by his process. He has employed the article, by the method of endermic medication, in some ten or fifteen cases, and can bear testimony to the potency of the preparation. The purer sample is shining, and almost perfectly white; the impure, of a nankin colour. There is no marked difference in their operation; both usually procuring sleep, when applied to a blistered surface, in the dose of the eighth of a grain. Sometimes, however, a quarter of a grain is necessary to produce the anodyne effect.—F. B.

69. *Discrimination of the Native Vegetable Alkalies.*—At a meeting of the French Institute, held on the 18th of February last, a memoir was read, on the application of iodine and bromine to the purpose of distinguishing the organic alkalies. These simple bodies, in vapour, are found to develop, in the alkalies referred to, distinctive colours, consisting of various tints of yellow, red, and violet. The different alkalies, merely distinguished by numbers, were sent by M. Pelletier to the author, who was enabled, by his method, to determine the name of each. The author expects to be able, by the aid of these reagents, to detect the presence of the organic alkalies in various mixtures, including those consisting of alimentary substances.

70. *Formation of Sulphuric Ether.*—M. Chevreul made a report to the French Institute, on the 10th of Dec. 1827, on a memoir, having this title, communicated by MM. Boullay, Jun. and Dumas. He sums up their results in the following propositions:

1. The authors have determined the elementary composition of the sweet oil of wine.

2. They have demonstrated, that this substance, by union with hyposulphuric acid, constitutes the sulphovinic acid.

3. They have confirmed by new analyses, the composition of alcohol and ether.

71. *Pyrothonide.*—This is the name given by Dr Ranque, chief physician of the Hôtel-Dieu at Orleans, to a product obtained by the combustion of hempen, linen, or cotton cloth. As it is coming into notice as a remedy for chilblains, chronic inflammations of the eyes, we give the directions for its preparation:

Take a handful of hempen, linen, or cotton cloth, either old or new. Place it in a shallow basin, and set fire to it in the open air. As the combustion proceeds, prevent the basin from heating too much. When it has ceased, reject the carbonaceous residue, and there will be found a semi-aqueous, semi-oily product, of a reddish and brownish tint, and possessing a penetrating, but not disagreeable odour. Pour upon this substance a glass of cold water, and spread the water by means of a bottle brush, over those parts of the basin where any of the oil may have formed, with a view to its complete solution. In this manner, a liquid is obtained, of a tint more or less deep, according to the quantity of the substance, produced by the combustion and dissolved in the water.

The substance proposed by M. Ranque would seem to be analogous to, if not identical with, that familiarly denominated oil of linen, and oil of paper.—*Nouv. Bibliothèque Médicale, Jan.* 1828.

72. *Sulphur in Assafœtida.*—M. Zeise has discovered sulphur in assafœtida. He proves its presence by the following means:—If assafœtida be treated with caustic hydrate of potassa and an acid be afterwards added, an effervescence occurs, and the gas disengaged blackens paper, wet with a solution of acetate of lead. If the drug be dissolved in alcohol, the solution evaporated to dryness, and the dry mass treated with nitromuriatic acid, a liquid will be obtained containing sulphuric acid. Finally, if the volatile oil of assafœtida, in a state of purity, be burnt, a strong smell of sulphurous acid will be perceived.—*Journal des Progrès, Tome VIII.*

73. *Cantharidin.*—M. Bretonneau communicated the following facts to the French Institute, at its sitting of the 24th of December last. His researches were undertaken to determine the relative quantity of cantharidin in different kinds of cantharides. He found those to possess the vesicating property in the highest degree, which belong to a species of the genus *mylabre*, to which M. Dejean has given the name *variabilis*.

M. Bretonneau's method of obtaining cantharidin is the following: Fill a glass tube, sealed at one end, one-third full with coarsely powdered cantharides

Add sulphuric ether, so as to cover them to the height of several lines. The tube, being well corked, is heated to 104° Fahr. Being allowed to cool,—as soon as the temperature falls to 86°, and the evaporation of the ether is longer to be feared, a ball of carded cotton is pressed into the tube, after the manner of a gun wad, and forcibly compressed by means of a metallic rod. In a moment the ether is absorbed, clarified, and evaporated, and leaves a fatty substance, which it had taken up from the cantharides. This substance is a coloured oil, sometimes combined with fat, to which it communicates a peculiar tint. It is to these two substances that the cantharidin is united; and when thus combined, it produces its epispastic effect. When this compound is mixed with a fixed oil, it communicates to the latter its vesicating property in a high degree. A piece of paper which has been made to imbibe an oil thus prepared, forms an excellent blister, which may be accommodated accurately to the shape of any part, however irregular it may be. The vesication thus produced is so exactly circumscribed, that the blister produced corresponds with the sharpest angles, which may be given to the paper employed.

74. *New Preparation of Copaiva.*—On the 26th January last, a communication was made to the Section of Pharmacy of the French Academy, by M. Miale, on a mode of solidifying this balsam, so as to bring it to a proper consistence to form pills. The method is as follows: Take a pound of the pure balsam, and an ounce of magnesia, strongly calcined. Place the balsam in a plate, and add the magnesia by means of a sieve. Mix and keep the mixture, guarded from the contact of air, for fifteen or twenty days. At the end of this time, the mixture will have acquired a consistence proper for pills, which being made of the weight of eight grains, will contain seven and a half of the balsam.

M. Miale states, that this preparation possesses the same powers as the balsam in its ordinary state. He also assures us that the employment of his process furnishes a means of determining the purity of the article. If it be sophisticated with castor oil, it acquires the consistence only of a syrup or ointment. The magnesia must be strongly calcined by a prolonged ignition.

Miscellaneous Articles.

New Theory of the Spleen.—Dr J. H. MILLER, professor of the practice of physic in the Washington Medical College, Baltimore, has addressed to the Editors of this Journal a letter, in which he authorizes them to make, on his part, the following statements.

Dr M. has come, by a series of observations and reflections, to conclusions resembling, in some points, those of M. JULES ARTHAUD, noticed in our last number. The mind of Dr M. was directed into the course of thinking which ultimately led to his present opinions, in the years 1808—9, and 10; in the last of which, he was engaged, together with the late Dr DORSEY, then adjunct professor of surgery, and Dr B. KUGHLER, demonstrator of anatomy, in a course of experiments, in part designed as repetitions of those of LE GALLOIS. From that period to the present, he has been occasionally occupied in gradually perfecting his new doctrines, which, in the session of 1827-8, he commenced delivering from his chair in the Washington Medical College. The engagements of a large practice in the country prevented their publication; but he communicated them to his fellow professors, in the year preceding that last named.

In compliance with the request of numerous professional friends, and especially of his class, Dr MILLER is engaged in preparing a work for the press, in which he will not only give an exposition of his principles, but also their application to the science generally. In the meanwhile, an outline of his views may be gathered from the following propositions, which we extract from his letter.

"Life depends on organization, and essentially consists in self-motion and self-reparation or sustentation. The grand moving principle is the nervous fluid or rather fluids. That this or these principles act through the medium of the nerves is not only inferred from analogy, but established by experiments and demonstrated by facts.

"There are three sets or systems of nerves.

"The first comprises the nerves of sensation, emanating from or centring in the cerebrum, the seat of thought or intelligence.

"The second,—those of motion, passing to or from the cerebellum.

"The third,—the ganglionic system or great sympathetic nerve, whose source or centre is the spleen or great secreting gland, analogous to the cerebrum and cerebellum of the two former, and which is the actuating power of all the involuntary motions, circulation, digestion, secretion, &c. &c.

"This last, according to the order of nature, ought to rank first; because it is the first formed, and exists in the inferior animals unprovided with the former. This is manifest from a slight observation of natural history, which abundantly shows that there is no animal without a sympathetic nerve, and that a cerebellum and cerebrum are only appended to the higher orders of vertebral animals.

"The ganglionic system is not supplied with a spleen, until the increased duties of the digestive and assimilative apparatus for the increased demand by the superadded organs of voluntary motion, sensation and thought, render it necessary

"Previous to the formation of the spleen, the ganglionic nerves, by means of an infinite number of radicles arising from the inner coats of the larger arteries, imbibe the first crude and ready formed fluid from the blood, and transmit it to the various destinations by the general radiating cords. These radicles continue to act in this manner, and are the chief instruments for imbibing the nervous fluid, even after the system acquires ganglia and a spleen. Hence it is that life may be sustained under disease of these organs, or after their removal, although in an obviously imperfect manner.

"The ganglionic system is distinct from and independent of the other nervous systems and centres. It arises neither from the brain nor spinal cord; but more strictly, they are indebted to it for existence, not directly by extension, but indirectly by secretion. Their twigs or branches never unite by anastomosis, however they may touch or inosculate.

It does enter the brain, not as a single cord, but in numerous ramifications, mostly in company with the arteries, to perform its office of secretion, &c. for the brain itself. It frequently meets with branches of the cerebral and spinal nerves, and travels in company with them, but still to perform its own offices.

"If we examine its anatomical structure and distribution, we discover that, far from originating from either cerebrum, cerebellum, or spinal marrow, its proper centre is located in the epigastric region, where we find its chief plexus (the solar) and its larger ganglia (the semilunar) between the crura of the diaphragm, over the spine and in contact with the spleen; from which it obviously arises in a thick coat surrounding the splenic artery, passing off to form the solar plexus, and thence divaricating in all directions, cerebral, lateral and pelvic."

Impediments in Speech.—At a meeting of the French Institute, held on the 10th of March 1828, MM. **Dumeril** and **Magendie** made a favourable report on a memoir by M. **Malbouche**, relative to the method of an American lady, named Leigh, for curing stammerers.

It is stated that this lady, after having examined all the French and English authors on the causes of stammering without any advantage, undertook herself to observe the most minute circumstances, connected with this infirmity. Having satisfied herself that she had discovered the immediate cause of stammering, she conceived a system of exercises for the organs of speech, by means of which she effected many radical cures. Afterwards a special institution was opened in New-York, in which, since 1825, more than 150 stammerers are said to have been cured.

The time necessary to effect a complete cure varies: it depends much less on the intensity of the malady, than on the moral energy of the individual affected. The longest treatments, however, do not require more than six weeks; while it is not rare for the cure to be effected in a few days, and even in a few hours.

The method in question, upon being introduced into the Low Countries, was attended there with the same success, and obtained rewards from the king, on the report of a commission.

M. **Malbouche**, authorized to make known this discovery in France, has demonstrated by positive facts, which the committee of the Institute has confirmed, that it is attended by the results announced. Letters from many stammerers, recently cured, have confirmed the good effects of the plan.

The committee, therefore, consider it as their duty, to extend their approbation to the method of Mrs Leigh; and they would have proposed that the Academy should give it publicity, if particular circumstances had not determined M. **Malbouche** to require that the details of the method should be kept secret.

Table of the Mortality in the Medical Department of the Philadelphia Alms-House.

Dates of Admission.	Women's Medical Wards.		Men's Medical Wards.		Men & Women's Surgical, and Women's Venereal Wards.		Men's Venereal & Eye Wards, Cells, and Obstetical Wards.	
1827.	No. admitt.	Deaths.	No. admitt.	Deaths.	No. admitt.	Deaths.	No. admitt.	Deaths.
June	62	5	69	13	54	3	59	3
July	76	7	78	8	57	4	57	3
August	70	8	110	14	70	4	65	4
Sept.	49	9	105	20	58	5	56	4
Octob.	58	8	150	3	79	3	66	4
Nov.	107	14	128	14	77	0	63	7
Dec.	61	13	93	17	68	3	52	1
1828.								
Janua.	49	9	96	11	48	2	50	3
Febru.	61	3	60	9	49	1	52	3
March	47	10	71	12	73	3	50	3
April	55	4	46	9	60	3	49	2
May	46	9	76	8	65	1	52	3
Totals	738	99	1082	138	768	32	671	40

RECAPITULATION.

Number of Women treated in Women's Medical Wards	738
Deaths	99
Percentage	13.4
Number of Men treated in Men's Medical Wards	1082
Deaths	138
Percentage	12.8
Number treated in Men & Women's Surg. and Women's Ven. Wards	768
Deaths	32
Percentage	4.14
Number treated in Men's Ven. & Eye Wards, Cells, and Obstet. Wards	671
Deaths	40
Percentage	5.96

Medical Patients treated	1820
Deaths	237
Percentage	13
Surgical Patients, &c. treated	1439
Deaths	72
Percentage	5

Total treated	3259
Deaths	309
Percentage	9.4

E. R. CHEW, Senior Student

Craniology.—There was presented at a meeting of the French Academy ot Sciences, held 31st December 1827, a work, which merits, say the French journalists, to fix the attention of the learned world.—The author is a Dr VIMONT of Caen, in Normandy, who, struck with the exact coincidences between the display of certain mental faculties and the development of the corresponding parts of the brain indicated by Gall, determined to institute a series of observations for himself. The result of six years' careful investigation and comparison is a magnificent collection of two thousand heads of the mammiferous animals and birds, in all of which he has found entire confirmation of the often ridiculed doctrine of craniology. He has shown, what many have denied in our own species, the strict relation between the prominences of the brain, the depressions formed by these on the internal surface of the scull, and the corresponding projections externally. This he has demonstrated by means of casts of the brain in wax, and sections of the scull in various directions. In other words, the prominences, or bumps as they are familiarly called, of the surface of the scull without, give the measure of the fulness of the particular portions of the brain within, and between those last and the propensities and habits of each animal thus studied, Dr VIMONT has discovered the most exact conformity.

In addition to a large number of brains moulded in wax from nature, there is, accompanying the work (a volume in quarto), an atlas of eighty plates, in which more than four hundred objects are delineated, with an anatomical accuracy and beauty of drawing hitherto unrivalled.

Circular—Medical Department of the Columbian College.—To complete the organization of the Medical Department of the Columbian College, a Chair, denominated *The Institutes of Medicine and Medical Jurisprudence*, has been established.

Professor RANDALL having been appointed to the newly created Chair, the vacancy occasioned by his resignation of the Professorship of Chemistry has been filled by THOMAS P. JONES, M.D., formerly Professor of Chemistry and Natural Philosophy in William and Mary College, and late of the Franklin Institute. By these arrangements, the scientific advantages of the Students are greatly augmented, without any material increase of expense, the graduation fee being reduced from thirty-five to twenty dollars.

During the last Course of Lectures, the Professors occupied their new Medical College, erected during the past Summer, and the result of the Winter's experience proves that the building is well suited to the purposes for which it was designed.

It is large and commodious, consisting of three elevated stories, with a roof peculiarly constructed for the admission of light into all the apartments appropriated to anatomical purposes.

On the ground floor is the Lecture Room, Laboratory, &c. of the Professor of Chemistry.

The second story contains the rooms, public and private, of the Professors of the Theory and Practice of Medicine and of Materia Medica, and of the Institutes of Medicine and Medical Jurisprudence.

In the third is the Anatomical Theatre, together with rooms occupied by the Professors of Anatomy, Surgery, and Obstetrics.

The theatre is designed from the most approved plans, and is conveniently connected with the rooms situated in the superior part of the building, which are intended for the purposes of Practical Anatomy. In relation to this particular department of the school, the professors have pleasure in saying, that care has been taken to provide space, light, and security, together with every other specific convenience that may afford to the Student facility in prosecuting to advantage this necessary part of his collegiate studies. Rooms have, also, been set apart for a Medical Library and a Museum of Anatomical Preparations.

Since the commencement of the last Session, the Professors have laid the foundation of a Library. Already medical works to a considerable amount have been collected, and provision has been made for its regular and speedy increase.

The Fifth Session of the Institution will commence on the first Monday in November next, and continue to the last of February. During this period, Lectures will be delivered daily, and full courses will be given on the various branches of Medicine, by

THOMAS SEWALL, M.D., Professor of Anatomy and Physiology.

JAMES M. STAUGHTON, M.D., Professor of Surgery.

THOMAS HENDERSON, M.D., Professor of the Theory and Practice of Medicine.

N. W. WORTHINGTON, M.D., Professor of Materia Medica.

FREDERICK MAY, M.D., Professor of Obstetrics.

RICHARD RANDALL, M.D., Professor of the Institutes of Medicine and Medical Jurisprudence.

THOMAS P. JONES, M.D., Professor of Chemistry.

The Ticket of each Professor is fifteen dollars; and all persons who have attended two full Courses, in this School, are entitled to attend succeeding Courses free of expense.

The requisites for graduation are similar to those required in the most respectable Institutions in the country. The candidate shall have studied three years under the direction of some regular physician. He shall have attended each Professor two full Courses, or he shall have attended one full Course in this School, and one in some other respectable Medical Institution. He shall have entered his name with the Dean as a candidate for graduation, and delivered to him an inaugural dissertation on some medical subject, thirty days before the close of the session.

The degrees are conferred under the authority of the Charter of the Columbian College, incorporated by the Congress of the United States.

R. RANDALL, M.D. Dean.

Writ for the Medical Convention of 1830.—Whereas the Convention that was held at the City of Washington, in the month of January 1820, for forming a Pharmacopoeia for our United States of America, did resolve that the President of that Convention should, on the 1st day of January 1828, issue Writs of Election to the several incorporated State Medical Societies, in the northern, middle, southern and western districts of the Nation, requiring them to ballot for three Delegates to a General Convention, to be held at Washington, on the 1st day of January 1830, for the purpose of revising the American Pharmacopoeia; and whereas the several Institutions, as aforesaid, are, by the same authority, requested to forward to the President, on or before the first day of April 1829, the names of the three persons so chosen; with sundry other provisions contained in the historical introduction to the work, to which the reader is referred.

Now therefore, I, SAMUEL L. MITCHILL, by virtue of the power vested in me by the Convention of 1820, do hereby give notice to all the incorporated Medical Societies, Colleges of Physicians and Surgeons, Medical Schools, and Faculties of Universities and Colleges, and all other authorized Bodies, that they choose proper persons to represent them in the General Convention to be held in January 1830, for revising the Pharmacopoeia.

Given under my Hand, this first day of January, 1828, at the City of New-York.

SAMUEL L. MITCHILL, President.

Bodies for Dissection.—" HOUSE OF COMMONS, *Monday, March* 24, 1828. Mr Dugdale presented the following petition from the Physicians and Surgeons of Birmingham, complaining of the inconvenience experienced by numbers of their profession from the want of bodies for dissection, and calling upon the House to remove the impediments which were at present thrown in the way of the cultivation of anatomical science.

" *To the Right Honourable the Commons, &c.*

" We, the undersigned physicians and surgeons of the town of Birmingham, respectfully implore the attention of your right honourable House to the urgent necessity of adopting some measures to remove the impediments to the cultivation of the science of Anatomy.

"Unfortunately deprived of legislative authority, exhumation is, at present, the only method by which bodies can be procured for anatomical purposes, a method painful to the feelings of the public, and opposed by the laws of this realm.

"In various parts of the continent of Europe the prosecution of the science of Anatomy is protected by their respective governments, and every facility afforded for its liberal and extended cultivation; hence, the youth of Great Britain, designed for the medical profession, are compelled to proceed to foreign countries in pursuit of that information which is denied to them here.

"As the practical surgeon is formed only by the laborious study of Anatomy and actual Dissection, your petitioners presume not to dwell upon the advantages to be derived by the public service and society at large, by attention to their prayer.

"Your petitioners most earnestly solicit your right honourable House to take this important subject into its serious consideration, and adopt such measures as in its wisdom may be deemed fit.

"And your petitioners will ever pray.

"Sir R. Fergusson thought that few, if any, such petitions had ever before been presented to that House; but he could not help thinking that the subject now called to their attention, perhaps for the first time, deserved the most serious consideration of the House. It was a matter of the most vital importance, and the difficulty of procuring subjects was at present so great, that he feared, if it were suffered to continue, the teaching of anatomy must stop, and the schools would be deserted for the want of means to offer instruction to the pupils.

"Sir J. Yorke said, he had presented some petitions of this kind in the course of last year, and he was glad that the attention of the House was now recalled to the subject. He wished for a declaratory law, the object of which should be, to let the public know that any one *in articulo mortis* was at liberty to sell his body for anatomical purposes, and to allow executors, administrators, and assigns, to give over the bodies of such persons to the purchasers. He was certain that if such a law was passed, there would be bodies in abundance; for there were few paupers who would not be glad to leave ten guineas to their widows; and the practice, thus introduced, would have this particular advantage about it, that those who went out of the world with any particular disease would be known to the medical profession, who would purchase their bodies, and thus be better enabled to ascertain the nature of that disease, and its effects upon the human frame, than they could possibly hope to do when, as at present, they were generally supplied with healthy subjects, that were only good for the purpose of teaching the younger pupils the common processes of dissection. He thought the House should state the law to be what he had mentioned; for now he believed it was generally considered that such a disposition of the body by the executor, although under the directions which the testator had given, was not authorised by law.

"Mr Warburton said, that in a short time he should move for a Committee to inquire into the subject which the petition had brought under the notice of the House. He had determined on doing so, in consequence of the particular request of some of the first surgeons in the metropolis. In a subsequent part of the evening, the Hon. Member fixed his notice of motion on this subject for Thursday next.

"Mr P. Thompson thought, that among the alterations of the criminal law, the House might introduce one which would materially assist in destroying the prejudice now commonly entertained against anatomical dissection. One of the concomitants of the severest penalty of the law was, in some cases, the handing over the body of the malefactor for dissection, and that circumstance tended to create a strong feeling of prejudice, which would probably cease to exist when that part of the law was altered. There was no such feeling in France, where bodies might be procured at ten francs each; in consequence of which there was no want of subjects for the schools, and the science of anatomy was taught under much greater advantages than in this country.

"The petition was brought up, read, and ordered to be printed."

On the 21st of April 1828, the London Medical Society held the following proceedings, relating to the same subject.

"Dr Clutterbuck pointed out, in a forcible manner, the disadvantages and impediments which at present exist in the cultivation of the science of anatomy; he observed, that at the time he was a pupil, subjects could be procured at the price of two guineas; whilst, at the present time, twenty guineas, or more, are paid, and the bodies are obtained with the greatest difficulty. The consequence of this, Dr Clutterbuck remarked, is, that students, with limited means, omit the study of anatomy, whilst others go abroad, where subjects are readily procured, and at a cheap rate. The worthy Doctor concluded by moving, that a petition which had been drawn up by the council, and intended to be presented to the Commons, should be read.

"The Registrar then read the petition, and a lengthened debate arose upon it; Mr Kingdon, Mr Cook (late of Exeter), and other members, objecting to the petition on the ground of its referring almost exclusively to the hardships experienced by *teachers*, in the want of subjects for dissection. It was at length moved by Mr Lambert, that a part of the petition to the above effect should be omitted, and this having been agreed to, together with some verbal amendments, the following petition was ultimately adopted.

"*To the Honourable the Commons of Great Britain and Ireland in Parliament assembled.*

"The humble Petition of the undersigned, the President, Council, and Fellows, of the Medical Society of London, established in the year 1773.

"Showeth,—That the perfection of medical and surgical science is founded upon an intimate and minute acquaintance with the structure and functions of the human body.

"That the requisite knowledge of anatomy and physiology can only be obtained by the frequent inspection and constant study of the dead human subject.

"That an adequate supply of dead subjects is indispensably necessary to enable the teachers of medical and surgical science, to explain to students the complex structure and varied functions of the animal frame in health, and the innumerable alterations effected in it by different states of disease.

"That the physicians and surgeons of this kingdom are altogether prevented from obtaining such adequate supply by the existing laws, which prohibit, under severe penalties, both pecuniary and personal, the appropriation of dead bodies generally, to anatomical purposes.

"That the facility with which subjects for anatomical purposes can be procured in all other countries compared with this, necessarily compels great numbers of medical and surgical students to go abroad for the purpose of prosecuting their professional studies.

"Your petitioners, therefore, humbly entreat your honourable house to take the matter of their petition into your gracious consideration, and to afford them such relief, as to your honourable house shall seem meet. And, &c.

"It was unanimously resolved, that the petition should be placed in the hands of Mr Warburton, and that it should remain for signature, at the rooms of the Society, until one o'clock of the following day; the time being thus limited, in consequence of the motion for a committee in the House of Commons, taking place on Tuesday night.

"On the motion of Mr Kingdon, a committee was appointed for the purpose of drawing up a petition, to be presented to the House of Lords; and it appeared to be the general wish of the Society, that this petition should be more comprehensive in its nature, than the one agreed upon for the Commons.

"Mr Alcock, on this point, very well remarked, that the leading feature of the petitions on this subject should be public benefit, and that the hardships and grievances of individuals, or classes of individuals, should not be dwelt upon.

"At the conclusion of the evening, a kind of conversational debate ensued, on the best means of supplying the schools of anatomy with subjects.

"Mr S. Wray stated, that it was his conviction, Parliament would give no remedy, and that medical men should themselves set an example, by giving up their own bodies for the purposes of dissection, and thus Mr Wray argued, the way would be paved, as it were, towards diminishing the prejudices of the public.

"Mr Lambert met these opinions by observing, that the notion of medical men giving up their own bodies, which had been trumpeted forth through many of the public papers, arose from a mistaken and narrow view of the subject. It was admitted on all sides, that no injury is done to the person dissected; the only injury committed, being upon the feelings of the relatives, or friends, of the deceased; hence the proposition was self-evident, that if the person submitted to dissection, had no friends or relatives, then no injury was sustained. He, Mr Lambert, was perfectly willing that his body after death should be dissected; but he could not forget, that there were those near and dear to him, whose feelings would be outraged, and this injury he had no right to inflict. Mr Lambert further remarked, that the annual amount of unclaimed bodies in the metropolis, as could readily be proved, would amply suffice for the supply of the anatomical schools; this appeared to him the only means that could possibly be adopted; to talk of diminishing the prejudices of surviving relatives, was, in his estimation, idle.

"Mr Kingdon concurred with Mr Lambert, with respect to dissecting unclaimed bodies; but, at the same time, he thought the public feeling might be well consulted, by making it compulsory, as in France, to give christian burial to the remains of the bodies, after the purposes of dissection had been served."—*Lancet, March 29, and April 26, 1828.*

TRANSLATION OF HUFELAND ON SCROFULA.

In press for the Trade by James Kay, Jun., and will shortly be published,

A Treatise on Scrofula. By C. G. Hufeland, Physician to the King of Prussia, and Counsellor of State.—Translated from the French of Bousquet, by Ch. D. Meigs, M.D. Member of the American Philosophical Society, &c.

Erratum.—The following error of oversight may be worthy the trouble of correction. In page 87, line 32, dele "[Erythronium dens canis?]" The plant alluded to is a species of Triticum, most probably *Triticum repens.*

THE

NORTH AMERICAN

Medical and Surgical Journal.

OCTOBER 1828.

Original Communications.

ARTICLE I.—*On Dropsy.* *By* D. FRANCIS CONDIE, M.D.

AMONG those diseases, the pathology of which is still involved in great obscurity, may be ranked dropsy. From his total ignorance of, or at best, his imperfect acquaintance with, the physiology of the exhalent and absorbent systems, it was not to be expected that the physician should have been able to form very correct views of the nature of their morbid conditions ; hence if we examine the various explanations which have been given of the formation of dropsical effusions, from the earliest periods of our science to a very late date, we shall find them, with few exceptions, to be mere vague hypotheses, presented without argument, and unsupported by a single fact. To enumerate the various opinions which have been entertained in relation to this subject, would indeed be a tiresome and useless task, —waiving it therefore, we shall enter at once upon what we believe to be a more interesting and profitable inquiry,—an inquiry into the morbid condition of the several tissues and organs of the body, with which the production of dropsy would appear to be intimately connected ;—for it is a fact, which every candid reasoner will be obliged to admit, that dropsy is more frequently to be viewed as the symptom of a disease, than as the disease itself.

By almost every medical writer of the present period, dropsy is supposed to result from an increased secretion, or a diminished absorption, of those fluids which are furnished, during a state of health, by the serous and analogous tissues. The proposition would, perhaps, be more correctly stated, were it said that dropsy depended upon a want of balance between the exhalation and absorption of the serous fluid of the cavities;—for although the increased exhalation is rendered evident by every symptom of the disease, the diminished absorption is by no means so clearly proved,—and that dropsy ever depends solely upon a diminished activity of the absorbents is a conclusion we cannot admit. That, in dropsy, absorption does not, in the affected cavity, equal in extent the amount of exhalation, is acknowledged; this must be the case, or the accumulation of fluid constituting the disease, could never take place;—but that, in dropsy, absorption is ever to a less extent than in the normal condition, has, as yet, never been attempted to be proved. That the action of the absorbents, generally, is not impaired, we know, from the fact that interstitial absorption goes on, in many cases, even to a greater extent than during health, as is evinced by the very great emaciation which is exhibited in those parts unaffected by œdema. Mercury and other remedies administered by friction, are absorbed in this as readily as in other diseases. Blood when effused beneath the cuticle is also removed by the absorbents with equal facility as under ordinary circumstances;—again, one serous cavity may be alone affected with accumulation, while in every other the exalation and absorption shall be in perfect equilibrum,—and even while every other part of the body is distended by a morbid collection of fluid, absorption will be found to go on, with perfect regularity, within the cavities of the joints, the synovial sheaths, the tunica vaginalis testis, &c. The general absorbent system, we may therefore infer, is not, during dropsy, in a state of diminished activity.

The only arguments which we have ever heard adduced in support of a diminished absorption in this disease, are drawn from the diminished exhalation from the skin, and the paucity of urine, two symptoms almost invariably attendant upon dropsy. Neither, however, have any connection with the circumstance they are brought forward to prove; neither the dryness of the skin, nor the diminished quantity of fluid discharged by the kidneys, has any relation to a diminished absorption;—both facts are to be explained by the augmented secretion which takes place from the surface of the serous membrane.

The external and internal exhalations are, in some measure, antagonists to each other; when the one is increased, the other being most generally diminished. This is strongly exemplified in the exhalations of the skin and those of the bowels, which are always in inverse proportion to each other; the same, by a parity of reasoning, we may presume to take place between the cutaneous exhalation and that of the serous membanes, particularly when the latter is in a morbid condition.

In regard to the small quantity of urine passed during the existence of dropsy, we are to remark that the amount of this excretion is, invariably, in proportion to the amount of fluids required to be removed from the circulating mass; and that whenever these fluids are discharged by any other outlet, whether by increased exhalation from the surface of the body, or by an augmented secretion into the intestines, the urine is always in diminished quantity:—when we consider, therefore, the amount of fluid poured out, in dropsy, by the serous membrane, we can very readily account for a diminution in the urinary discharge. "Urina idcirco parcè redditur," says **SYDENHAM**, "quia sanguinis serum, quod per ductus urinarios ex lege naturæ excerni debuerat, jam in cavitatem abdominis deponitur, &c*."

It is to be recollected, however, that, even during the existence of dropsy, we have no reason to presume the saline matters secreted by the kidneys, to be less in amount than under ordinary circumstances.

The rapidity with which the fluid reaccumulates, in cases of ascites, after tapping, is another argument against the supposed origin of dropsy from diminished absorption. In the case of a female, labouring under ascites, which came under our notice; from the 24th of February 1825 to the middle of October of the ensuing year, the operation of paracentesis was performed thirty-five times, and the fluid drawn off amounted to one hundred and eighty-six gallons:—the usual interval between the operations was two weeks, and the quantity discharged each time, about five gallons. It is impossible to believe that so great an amount of fluid could be produced, in so short a period, by the ordinary action of the exhalents, even if we were to suppose the absorbents of the cavity to be totally inactive.

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* Tractat. de Podag. et Hydrop. p. 70.

From what has been said, we may, I think, safely set out with the proposition, that dropsy depends upon an augmented exhalation of the fluids furnished by the serous and analogous tissues ;—so far, clear and positive facts bear us out, but whether a morbid condition of the absorbents ever concurs in the production of the disease, we have as yet no means of judging.

The next question that presents itself is, upon what does this increased exhalation depend ? Every one who is conversant with the writings of the British physicians, is well aware that according to the pathological views of a majority of them, even to the present day, dropsy is considered to be a "disease of debility." What is exactly to be understood by a disease of debility, it is not very easy to determine,—the expression itself conveying no definite idea ;—that, however, it is employed by these writers to indicate a condition entirely opposed to the existence of inflammation, or of increased action of any kind,—something which entirely precludes the use of depletion, we are well aware, from the fact of their either forbidding the employment of the lancet and the abstraction of blood in any manner, or recommending it with all the precaution requisite in the most dangerous experiment,—restricting its use to relieving certain distressing or immediately dangerous symptoms, and scarcely ever admitting it as a part of the curative plan. In accordance with the genuine spirit of this doctrine, Dr Good remarks, upon the plan of treating dropsy by purgatives and diuretics, that "it should be regarded as a preparatory step only ; a mere palliative ; and an evil in itself, though an evil of a less kind to surmount an evil of a greater."—"Every purgative that does not diminish the general bulk, adding to the general disease by increasing the debility*."

If by denominating dropsy a disease of debility, nothing more were intended than that, in many cases, it is accompanied by a paleness of the surface, diminished activity of the voluntary muscles, and a diminished action of the heart and arteries, while we reprobated the admission of such laxity of expression into pathological language, we should have no objection to acknowledge, that to a certain extent the statement is true ; and we shall hereafter attempt to shew that the condition of the circulation and of the muscles referred to, may, in some instances, be even a cause of dropsical effusion. We apprehend, however, that what most of those who define dropsy to be a disease of debility mean by the expression, is that it owes its origin to a laxity, weakness, or diminished tone of the exhalents concerned in its production,—or as a late writer has explained the theory, "to a relaxed state of these vessels, which *open their mouths too widely*, and suffer the serum or other aqueous fluid to escape with too much freedom*."

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* Study of Medicine, Fourth Edition, Vol. V. p. 385.

The advocates of such a doctrine, must certainly suppose the serous fluid, which lubricates the various cavities, to be separated mechanically from the blood,—that it oozes, as it were, through orifices which are so adapted in size as only to admit a certain quantity to pass, until these orifices are enlarged by disease. Such views of exhalation cannot, however, be admitted in the present state of physiology. The function of the exhalents is to furnish a fluid of a particular kind, adapted in quantity to answer certain purposes;—when, therefore, we find this fluid furnished in an augmented quantity, the conclusion is evident,—the exhalents have been increased in activity. If the serous fluid given out by the exhalents was accumulated in certain reservoirs, and the exhalent orifices acted merely as sphincters to prevent its discharge excepting under certain circumstances, or to a certain amount, we should then understand in what manner their relaxation, their *gaping open*, would allow an increased discharge of serum to take place. But viewed in the light of organs adapted to separate from the blood a certain product, let them at any time sink into a state of weakness, or of relaxation, and the only effect that could result would be a diminution of that function, never its augmentation. We therefore conclude, that in every case of dropsy there is an increase of activity in the exhalents. But, as has already been hinted, this increase of activity is, in almost every instance, an effect of previous disease of one or more of the tissues or organs of the body, upon the removal of which, when practicable, the cure of the dropsy depends. Hence, it will be perceived, that, in a practical point of view, a consideration of the several diseases productive of dropsical effusions, is a subject of very considerable interest; in relation to it, therefore, we propose in the present essay to offer a few general remarks. And first of inflammation.

An increased serous effusion is the result of inflammation. Although this is not laid down as a universal proposition, to which no exceptions can be adduced, yet it will be found, upon examination, to be more universally true than is commonly supposed.

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* Good, Study of Medicine, *loc cit.*

Laennec has shewn that inflammation of the pleura is invariably accompanied by an increased effusion of serosity into the cavity of the chest, and the same can be proved to be the fact in relation to every inflammation occurring in the serous tissue. The larger joints, the cavities of which are lined with a membrane almost identical with the serous, seldom, if ever, become inflamed, without being distended by a morbid serous effusion; the same increased secretion of serum is found in inflammations of the tunica vaginalis testis, of the pericardium, &c. It is indeed surprising, that this dependence of dropsy upon inflammation should not, until of late years, have more generally attracted the attention of physicians; particularly when they had, every day, under their notice the fact of an increased effusion of serum, resulting from the inflammation of a blister, of erysipelas, of violent friction applied to the skin, of burns and scalds, &c. They were probably, however, not aware that the effusion in these latter cases is precisely the same as that constituting dropsy.

To Dr Rush is certainly due the credit of having first pointed out to the physicians of our own country the intimate connection between certain dropsies and inflammation; and of founding upon this pathology a more rational and successful practice than that previously pursued. But he was by no means the first medical writer who was aware of this fact. Monro had, previously, inculcated the necessity of direct depletion, in all cases of dropsy attended with the same general symptoms as were then supposed to be the only indications of an inflammatory disease. Stoll, likewise, in his "*Prælectiones in Diversos Morbos*," published in 1788, describes an inflammatory dropsy, to be removed by antiphlogistic and emollient remedies. The same fact appears, indeed, to have been noticed from the earliest periods of medical history, as we infer from the circumstance of blood-letting having been considered an essential remedy in certain cases of dropsy, by Hippocrates (*de Dieta in Acutis*), by Galen, Alexander of Trailles, Paul of Eginetus, Riverius, Botallus, Etmuller, Mead and others. The frequent connexion between dropsy and inflammation is, likewise, confirmed by a reference to numerous cases, with their examination after death, recorded by Morgagni and Lieutaud; and to the works of Van

Sweiten, which contain frequent examples of dropsy occurring during or immediately subsequent to many of the phlegmasiæ.

But it is almost entirely from the writings of physicians of a very late period, that any very definite facts can be collected in support of this point. Previously, even by Dr **Rush**, dropsy was spoken of in a very vague manner, as depending upon, or as connected with an inflammatory, or phlogistic condition of the system, and although the treatment founded upon opinions, thus loosely expressed, was, in its general outlines, correct, yet it could, by no means, be so well adapted to individual cases, or so generally successful, as when, from a closer attention to the symptoms of the disease, in connection with the facts derived from morbid anatomy, we become better acquainted with the seat, nature and extent of the inflammation by which it is produced, and are enabled, in consequence, to apply and modify our remedies accordingly. The same objection of vagueness which has been attributed to the language of **Rush**, and of preceding and contemporary writers, on the subject of the inflammatory character of dropsy, may be applied, with nearly equal propriety, to that of Dr **Parry**, who was among the most zealous of the modern defenders of this doctrine in Great Britain.

Cruickshank, though, comparatively, an old writer, appears to have been well acquainted with the connexion which subsists between dropsical effusion, and inflammation of the serous membrane. "The second species of dropsy," he remarks "is very common;—it is that which arises in consequence of previous inflammation of a cavity (those of course lined by the serous tissue), and may take place in any habit of body. If an inflammation take place in a cavity, it may terminate in a number of different ways; one of these is by an increased secretion of the fluid of surfaces. A man receives a blow on the testicle, inflammation takes place, and the consequence is, frequently, a hydrocele, or dropsy of the tunica vaginalis. A child's brain inflames, and this inflammation ends at last in hydrocephalus, or collection of water in the brain. In like manner pleurisy frequently terminates in hydrothorax, or collection of water in the chest. I have often," he adds, "taken away forty to sixty pints of water, which had accumulated in the cavity of the abdomen, in the few days the peritoneal inflammation had lasted, during the usual species of child-bed fever. This is to be considered as substituting a less dangerous disease for another;—peritoneal inflammation killing often in three days, while ascites may last for twenty years*." Precisely the same opinion, in regard to the formation of abdominal dropsy, was maintained by **Pemberton**, who viewed it as a species of natural cure of peritonitis; his words are, "sometimes a resolution of the inflammation takes place, when ascites is produced†." From his morbid anatomy, we find that Dr **Baillie**, likewise, held similar views in regard to the formation of hydrothorax.

Laennec, whose name is in itself a host, speaking of dropsy of the chest, after having noticed an increased secretion of serum as an invariable effect of pleuritis, pericarditis, &c. remarks, that "whatever may be the difference, both in the general symptoms and the organic lesion, between a case of hydrothorax and an acute pleurisy, or between a case of ascites from general debility or organic disease of the heart or liver, and the same disease from an attack of peritonitis; or in short, whatever may be the difference, in general, between a dropsy and an inflammation,—there can be no doubt that these two affections, so opposite in their extreme degrees, are, nevertheless, often very nearly allied in their slighter shades:—we frequently find albuminous concretions amid the serum of ascites, and purely serous effusions in inflammatory affections of the chest‡."

Acute dropsy of the pericardium, says **Itard**, like all other acute dropsies, is almost invariably the result of an inflammation, and consequently is almost always symptomatic. It is more especially an inflamed condition of the pericardium, or of a neighbouring portion of the pleura or lung, that we are to regard as its most ordinary cause§. The general dependence of this variety of dropsy upon pericarditis had been previously insisted upon by **Pinel**.

So well convinced was **Hunter** of the connexion of dropsical effusion with an inflamed condition of the vessels of a part, that he has enumerated, among the varieties of inflammation, one which he denominates the œdematous; the distinguishing symptom of which is an extravasation of serum into the cellular membrane at the part affected‖. A similar species of inflammation is described by **Jadelot**, in his work on the diseases occurring in the Hospital for Sick Children at Paris.

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* Anatomy of the Absorbent Vessels, Second Ed. 4to, 1790.

† Diseases of Abdominal Viscera, Fourth Ed. p. 57.

‡ De l' Auscultation Mediate, &c. Tom. I.

§ Sur les Hydropisies, Paris, 1818.

‖ Treatise on the Blood, &c. Philad. Ed. p. 238.

That hydrocephalus owes its origin to inflammation of the membranes of the brain we have the most conclusive evidence. "Nothing more occurs in this disease," observes Dr Rush, "than what takes place when hydrothorax follows inflammation of the lungs, or where serous effusion follows inflammation of the joints."—In one hundred and seven individuals who fell victims to an inflammation of the membranes of the brain, observe MM. Parent and Martinet, in a late work*, the brains of sixty-seven presented, upon examination, an effusion of serum to a greater or less amount.

The foregoing facts and opinions are fully confirmed by the experience of every medical writer, who has presented us with a pathological history of inflammation of the serous membranes, based upon morbid anatomy.

To be convinced of the inflammatory origin of dropsy, it is only necessary, indeed, to examine after death the cavity in which the morbid accumulation is seated. Thus in hydrothorax and ascites, the pleura and peritoneum will, in very many instances, be found not only highly vascular, but deprived of their natural transparency and glossy appearance, and covered, to a greater or less extent, with patches of coagulable lymph. Occasionally, it is true, so great an interval may have elapsed between the existence of the inflammation and the fatal event, as to render it somewhat difficult to recognize the more decided marks of its previous existence ; but even in such cases, the thickening of the serous tissue, and the presence of portions of false membrane adhering to the surface of the cavity, or of shreds of coagulable lymph floating in the effused fluid will indicate, with certainty, that inflammation must have existed. It is very correctly remarked by Portal, that a post mortem examination has frequently shown the existence of inflammation in cavities affected with dropsical accumulation, even when no such inflammation has been indicated during the life of the patient, or, at least, its symptoms have been so obscure as to have escaped the attention of the medical attendant†.

The following conclusions, the correctness of which is established by the result of numerous pathological observations, very readily explain the formation of what have been termed acute dropsies.

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* Sur l' Inflammation des Meninges, &c.

† Observations sur la Nat. et le Trait. de l'Hydrop. Tom. I. p. 62.

Whenever a serous membrane is attacked with acute inflammation, the action of its exhalents becomes altered, and in place of the almost imperceptible halitus which lubricates the surface of the membrane during a state of health, they secrete an albuminous fluid, which, immediately upon its being discharged from the vessels, separates into two distinct parts. The one solid, answering to the coagulable lymph of HUNTER, and the other fluid. The solid portion adheres, for the most part, to the surface of the membrane, and becoming, subsequently, penetrated with blood-vessels, constitutes what have been termed false membranes and the medium by which morbid adhesions are produced between the contiguous parts of a cavity;—while the fluid portion is, in general, taken up by the absorbents as quickly as it is formed. The above constitutes, in fact, the process by which a natural resolution of most acute inflammations of the serous membrane is effected. In certain cases, however, it happens that the serous portion of the morbid secretion is so great in amount, that the absorbents are incompetent to its immediate removal; a portion of it remains of course in the cavity in which the inflammation is seated, and constitutes the foundation of a dropsical accumulation, which latter never fails to be fully formed, provided the inflammation upon which it depends be not speedily and effectually removed.

The amount of serous fluid furnished by the exhalents in phlegmasiæ of the serous membrane, would appear, very generally, to be in proportion to the extent of surface occupied by the disease, and, also, to the slowness with which the inflammation runs through its several stages. When the inflammation extends to a small portion of the membrane only, and runs a rapid course, then effusion of coagulable lymph, or adhesions between contiguous surfaces is the most common result,—but when the disease occupies nearly the whole surface of a cavity, and runs more slowly through its stages, we have an abundant secretion of a serous fluid, while but little coagulable lymph is effused.

The foregoing remarks are more particularly applicable to inflammation of the pleura and of the peritoneum. When the serous membranes of the brain become inflamed, the effect is, generally, a morbid secretion of a limpid fluid, containing so small a portion of albuminous matter, as scarcely to undergo any degree of coagulation upon the addition of an acid, or by exposure to heat. It is certain that in many cases of arachnitis coagulable lymph is effused to a very considerable extent,—but such is by no means so common an effect of inflammation of this membrane as the increased serous discharge alluded to.

From what has preceded, we find, therefore, that in every instance in which inflammation is seated in the serous tissue, there is more or less tendency to the formation of a dropsical accumulation; and that STOLL was perfectly justifiable, when, in his aphorisms, he placed dropsy among the several terminations of the internal phlegmasiæ.

The frequent dependence of dropsy upon inflammation might be still further established by an examination into the external causes to which it succeeds, the symptoms by which, in many cases, it is accompanied, and the treatment most effectual in its removal;—but what has been already said, we conceive to be sufficient upon this point. The fact which is so very generally admitted by the physicians of this country, is daily gaining credit among the profession in Europe. It has been maintained in Great Britain by PARRY, WELLS, BLACKALL, AYRE, and the Edinburgh school generally;—in France by DUPUYTREN, MARENDELL, ITARD, BRESCHET, PORTAL, &c. and in Italy by GEROMINI and others.

Our remarks, heretofore, have been almost exclusively confined to acute inflammation; but dropsical accumulations are still more frequently the result of inflammation in its chronic form. This variety of disease, but little understood, and too often entirely overlooked, by physicians, before they were made acquainted with its true character and symptoms by the writings of BROUSSAIS, in perhaps the majority of instances, when seated in the serous tissue, terminates in a morbid effusion within one of the great cavities. The histories of chronic pleuritis and peritonitis, as detailed by LAENNEC and BROUSSAIS, will sufficiently evidence this fact.

Ovarian dropsy we believe to be, in every instance, the result of a chronic inflammation of the substance of the ovary, which after continuing for a longer or shorter period, extends finally to its serous envelope. This is likewise the opinion of Dr AYRE, who has recently published a very valuable treatise upon dropsy. It is to be remarked, that in cases of ovarian dropsy, the fluid discharged by puncturing the diseased organ, is never of so serous and limpid a character as that contained in the abdomen in ordinary cases of ascites; but is either a thick glareous matter of various colours, or a turbid fluid, containing portions of a membranous appearance, pus, albuminous floculi, &c. and further, that the disease never continues for any length of time without producing an effusion of serum within the cavity of the abdomen. In every instance of ovarian dropsy which has come under our notice, the effects of extensive inflammation of the peritoneum were exhibited, upon examination after death.

Dropsy from chronic inflammation is too often the consequence of mismanagement on the part of the practitioner:—an acute affection of the thorax or abdomen, treated by improper remedies, or without sufficient vigour, in its first stages, may be deprived of its acute character, but still continue, masked under a chronic form, to produce the most extensive ravages,—the more fatal from their true character being unsuspected. This is the source of very many of those dropsies which succeed to violent febrile affections:—dropsies which have been ascribed, almost invariably, to an exhaustion of the constitution from the preceding disease.

We find, says **Portal**, in his treatise on dropsy already quoted, in the writings of **Morgagni**, **Senac**, **Lieutaud** and other celebrated authors, such frequent examples of dropsy succeeding to fever, whether continued, remittent or intermittent; they are indeed of so common occurrence; that we should be always upon our guard to prevent their formation, by the prompt and entire removal of the febrile affection; for the success of our remedies in these dropsies, after they have taken place, is always exceedingly doubtful. On opening the bodies of those who die, he adds, in the cases here alluded to, we are presented with engorgements, obstructions, and even suppurations of the viscera. In another part of the same work, he remarks, in relation to dropsy succeeding to an inflammation, that so far from an abuse of blood-letting giving rise to it, as has been supposed by some, it is frequently to be referred to the circumstance of the evacuation of blood not having been carried to a sufficient extent, in consequence of which, an engorgement of the inflamed organ is permitted to remain, or to take place, at the very time when the patient is supposed to be upon the recovery. The truth of this important remark might be verified by a host of examples.

Although we are inclined to refer the production of dropsy, in a majority of instances, to pre-existing inflammation, yet we are persuaded that every case cannot be traced to a similar source.

Among the causes of dropsy independent of inflammation, we may mention an impediment to the free circulation of the blood, particularly in the veins. By consulting the records of our profession, we shall find it was long believed, that whatever prevented the free return of the blood to the heart would occasion a morbid effusion of serum into the cellular membrane, or one or other of the great cavities;—lately, however, the fact has been as positively denied, as it was before strenuously contended for.

One of the principal evidences in proof of serous effusion as a result of obstructed circulation, has been drawn from the experiments of **Lower**. This gentleman, towards the middle of the seventeenth century, finding that a serous infiltration occurred in the extremity, upon the upper part of which a ligature had been applied, was induced to ascertain whether the same effect would result when one of the principal veins of the body was tied. He accordingly applied a ligature upon the vena cava of a dog, immediately below the diaphragm, and found that the abdomen and posterior extremities became dropsical. In another experiment, when the two jugulars were included in a ligature, an effusion of serum was very rapidly produced in the cellular membrane of the face and neck of the animal; and in a third, hydrothorax was produced by a ligature of the vena cava, immediately before it enters the right auricle. Notwithstanding these experiments are liable to many objections, they were urged by **Boerhaave** as conclusive, and we are informed, that they were repeated by **Bonetus** with every precaution, and with a similar result. That the conclusions drawn from them correspond with a variety of pathological facts we have no hesitation in asserting. Take an individual, the compositor, for example, in a printing office, who is obliged, for the greater part of every day, to remain standing in one position, and we shall find, especially when from intemperate habits, or other cause, the tone of his general system has been reduced, that very soon he will be affected with œdema of the feet and ancles, which, at first, disappears during the horizontal posture assumed at night, but subsequently becomes permanent and gradually ascends to the legs. In this case we have several causes tending to impede the venous circulation in the lower extremities,—the effect of gravity upon the ascending column of blood, from the constant erect position of the body,—the want of muscular motion, &c. We know also, that individuals, thus circumstanced, are precisely those in whom a varicose distention of the veins of the legs is of most frequent occurrence.

There is, in fact, scarce a person, who has passed the meridian of life, unless he possess an unusual share of constitutional vigour, who is not, towards the close of the day, affected with an œdematous swelling of the feet, to a greater or less extent, and a very evident over distention of the veins of these parts. Few old people, it is observed by **Portal**, can be found who do not labour under œdema of the legs:—the same author notices, also, the frequency of these swellings in individuals who make long journeys in a carriage;—in both instances, he adds, they are occasioned by the impediment the blood experiences in its return to the heart; they very readily disappear upon the patient's assuming the horizontal position at night; and by proper exercise during the day, are prevented from returning*.

It may, however, be objected, that in the instances above referred to, the impediment is not confined to the passage of the blood in the veins, but extends equally to the contents of the lymphatics. This objection, although it does not appear to us to be of much weight, may be met by a reference to those cases in which the impediment is confined strictly to the circulation of the blood. Thus, hydrothorax is produced by malconformations of the heart, &c. Numerous observations collected by **Morgagni**, **Bonetus**, **Lieutaud** and others, had already demonstrated the intimate dependence of morbid effusion within the chest upon aneurismal swellings of the ventricles or auricles of the heart, polypus concretions within its cavities, augmented size of its parietes, &c. but it is more particularly to the researches of **Corvisart** and subsequent writers, that we are indebted for any clear information on this subject. From these authorities we learn, that the derangement of the circulation, arising from organic lesions of the heart, the slightest in appearance, as for example, ossification of one or more of its valves, or even a simple disproportion between the capacity of its cavities, occasions, in almost every instance, a dropsy of the thorax†. The same effect is produced by a varicose dilatation, or an obstruction, compression or obliteration of the pulmonary veins, consequent upon certain chronic affections of the lungs. Aneurism of the aorta, malconformations of the thorax, &c. are, likewise, well known causes of hydrothorax.

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[*] Op. citat. Tom I. p. 204.

[†] Sur les Maladies et les Lésions Organiques du Cœur, &c.

But cases have been lately recorded, in which the obstruction is still more strictly confined to the venous circulation. I allude to those instances, in which, from inflammation of the lining membrane of a vein its cavity is obliterated, or the circulation through it is impeded in consequence of the formation of coagula. The cases of **Davis**, in the Medico-Chirurgical Transactions, are of this character; for although the relator has denominated them instances of plegmasia dolens, they were evidently nothing else than anasarcous swellings of the lower extremities, resulting from obliteration of one or more of the large venous trunks, in consequence of preceding inflammation. This conclusion is confirmed by the phenomena of a case related in the 13th volume of the last mentioned work by Dr Forbes. A journeyman printer, twenty-six years of age, had the whole of the left lower extremity enlarged to double its natural size;—the swelling extended from the foot to the groin;—it every where pitted upon pressure, presenting an appearance perfectly similar to that observed in common anasarca. The sub-cutaneous veins were distinctly marked, being distended with blood.' The patient died; and on examining the diseased limb, the cellular tissue was found, over its whole surface, very much distended with a limpid fluid; the veins were tense and filled with coagulated blood;—the superior portion of the femoral vein, as well as the external and common iliacs, were found filled with a firm coagulum having much of the fibrous appearance of the coagula contained in aneurismal sacs:—on separating the coagulum, a thin but distinct membranous layer was seen adhering to the internal coat of the vein, requiring some force to separate it. **Morgagni** relates, also, the case of a female whose left lower extremity was œdematous, in which, after death, he found the left lower iliac vein filled with a coagulum, as he terms it, of a polypus nature, the canal being entirely obliterated by it; while the crural vein was at least one third narrower than natural, and appeared to have been in that state for some considerable time. (*Epis*. lvi. *Sec*. 10.) Another case is recorded by the same author (*Epis*. l. *Sec*. 55) of a somewhat similar character.

In the body of a man who had been affected with œdema of both the lower extremities, **Raikem** found the inferior cava, above the emulgents, filled with a concrete, fibrous matter of a whitish colour. In another instance of dropsy of the lower extremities, but particularly of the right, recorded by the same writer, the inferior cava, towards its bifurcation, and the left primitive iliac were filled with a reddish matter, analogous to the fibrine of the blood somewhat altered, while the right primitive iliac was impervious, being converted into a ligamentous chord.

In the Archives Generales, Tom. II. and IV. **Bouillaud** has collected a great variety of facts upon this point. In one instance to which he refers, an œdematous swelling of the right side of the face and neck was found to be connected with an obliteration of the internal jugular, and of all the venous branches which terminate in the right subclavian :—in another, in which an anasarca, at first confined to the upper extremities, extended, subsequently, over the whole body, the two cavæ were almost entirely obliterated by coagula of a fibrous appearance: in two other instances referred to by M. **Bouillaud**, ascites resulted from an obliteration of the vena portæ; the cavity of the vein, in the one, being filled with a solid fibrous coagulum, in the other, by a coagulum of a fibrous appearance, but of a pulpy consistency.

In a case reported by Mr **Travers**, after the operation for popliteal aneurism, in which the femoral vein, as well as the artery, was tied, a serous infiltration of the leg quickly ensued*. Mr **Hodgson**, likewise, relates an instance of dropsy of the lower extremities from obliteration of the femoral vein, while **Rhodius** refers to an instance of ascites, in which the descending cava was so much contracted, as scarcely to allow the passage of a middle sized probe into its cavity.

Every affection of the lungs by which the circulation through them is rendered difficult, is also attended with morbid effusion within the chest. Thus in cases of hepatization, schirrhus, tubercles, fungus hæmatodes, melanosis, and various other affections of these organs, more or less of a serous fluid is invariably effused into the cavity of the pleura, and the death of the patient is, frequently, preceded by the most urgent symptoms of hydrothorax, while upon examination after death the pleura exhibits no symptoms of an inflammation having existed, to which the formation of the serous accumulation could be attributed.

The foregoing facts are, we conceive, sufficient to prove that a congestion of the capillaries, consequent upon an impeded circulation through the returning vessels of a part, is capable of rousing the exhalents to an increased secretion. But it is proper here to remark, that morbid effusion of a serous fluid is not a *necessary* and *invariable* consequence of the passage through a principal vein being suspended;—thus, Mr **Hodgson** reports a case in which the femoral vein was tied, and Mr **Simmons** of Manchester, another, in which the same operation was performed upon the internal jugular[*], and in neither instance, we are informed, was any degree of dropsical effusion produced. **Rayer** and **Bricheteau** tied, in rabbits, the abdominal cava, above the origin of the emulgents, the primitive jugulars, the axillary veins, &c. and, but in one instance, found that the impediment to the circulation thus produced, gave rise to any considerable degree of serous effusion[†]. **Travers** reports, upon the authority of **Wilson**, a remarkable case, in which, not only the whole extent of the vena cava, above the origin of the hepatic veins, but also the emulgents, the spermatics, the primitive iliacs and their principal branches, were found filled with lymph and coagula of considerable size, without any appearance of serous effusion having resulted, in the limbs or other parts of the body[‡], and Dr **Baillie**, in his observations on the obliteration of vessels, relates a somewhat similar case[§]. It is from analogous facts that **Hodgson** concludes dropsy very rarely to result from the obliteration of a principal vein[||], which is also the opinion of **Beclard**; while **Bichat**, who, as we know, refers every disease to an alteration in the vital condition of some portion of the system, has strongly combated the supposition, that dropsy, in any instance, arises from a mechanical, or other impediment to the free course of the venous blood. To this sweeping conclusion, we cannot, however, subscribe; we have already referred to instances of serous effusions intimately connected with the very cause here denied ever to produce them, and many more might be cited. During pregnancy, particularly in its advanced stages, what is more common than an œdema of the lower extremities? and to what cause can we ascribe this latter, other than the pressure exerted upon the vessels within the pelvis, by the pregnant uterus? The condition of the veins of the extremities is a sufficient proof, that, under the circumstances alluded to, the circulation through them is impeded. It is stated by **Rayer**, in evidence of the œdema in these cases resulting from the pressure of the gravid uterus upon the vessels, that when there occurs during pregnancy a lateral obliquity of the uterus, the œdema is confined to the side corresponding with that to which the uterus is turned.

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* Cooper and Travers's Surgical Essays, p. 179.

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[*] Medical Facts and Observations, Vol. VIII. p. 23.

[†] Sur l' Hydropisie. Paris, 1825.

[‡] Vide Medico-Chirurg. Trans. Vol. III. p. 65.

[§] Ibid. Vol. I. p. 127.

[||] Vide Diseases of Arteries and Veins.

Œdema of the feet and legs is likewise an effect of the pressure from aneurismal tumours seated in the ham or groin; while an axillary aneurism, in like manner, occasions an œdematous swelling of the hand and arm of the side upon which it is seated.

In cases of scirrhus or cancer of the female mammæ, when the glands of the axilla become enlarged, the entire arm of the affected side becomes most generally anasarcous. In lumbar abscess, also, one of the very first symptoms is, not unfrequently, an œdema of the extremity of that side upon which the collection of matter is seated, which continues until the abscess finding its way to the surface no longer compresses the vessels of the limb. The same is equally true of most deep seated collections of matter:—in these an œdema being, for a long time, the only symptom of their existence.

"Other circumstances," says RAYER, "which relax the course of the venous blood through the vessels of the extremities, give rise frequently to œdema. Thus, a varicose condition of the superficial or deep seated veins of the leg, is many times accompanied with a watery intumescence of the feet and ancles: examples of this have been collected by M. BOULLAUD, and," he adds, "there is not perhaps a single physician who has not observed the same."

It was remarked by RUYSCH, that a varicose condition of the spermatic veins, very commonly precedes the formation of hydrocele. MORGAGNI likewise (*Epis.* xliii. *Sec.* 34) was of opinion that the latter disease may be produced by the existence of an obstacle to the free return of the blood from the testicle:—certain it is, remarks POTT, that the spermatic veins are very often found varicose in persons afflicted with hydrocele of the tunica vaginalis; while, accordding to RAYER, the greater frequency of hydrocele of the left side is, undoubtedly, connected with the fact of varicocele occurring in a majority of cases, in the veins of that side.

The last mentioned writer notices, likewise, the circumstance of inguinal and crural herniæ producing, frequently, œdema of the foot and leg, by the pressure which they exert, when they have attained to any considerable size, upon the vessels of the limb. This, according to RICHTER, is more commonly the case in omental hernia, and is attributed by him to the pressure upon the vessels from a portion of hardened omentum being greater than that produced by a hernial sac containing only intestine. A case is referred to by Hannemann, of a female labouring under omental hernia, with extensive œdema of the foot, which had continued without abatement for many months, but quickly disappeared after the reduction of the hernia.

Œdematous swellings are also very commonly symptoms attendant upon extensive tumours, the result of inflammation, but of an indolent character, situated in the neck, axilla, groin, ham, upon the leg, &c.* In some of these cases, however, we apprehend that the morbid effusion of serum is not to be ascribed, solely to compression of the venous trunks, it being more than probable that the extension of irritation to the surrounding cellular membrane may concur in causing the increased action of the exhalents.

Is abdominal dropsy ever produced by an obstruction to the portal circulation, arising from a diseased condition of the liver? This is admitted by a great majority of the writers on dropsy; but by Bichat and others, is replied to with an absolute negative. Bichat, in particular, maintains, that the obstruction here supposed cannot possibly take place, in consequence of the free communication kept up between the two cavæ by means of the vena azygos†; and Dr Ayre who, in his late very valuable treatise, refers all dropsical effusions, in every instance, to a particular grade of inflammation, asserts, that we have no satisfactory instances yet shown to us, of such a precise condition of the liver as has been supposed to interrupt the freedom of the circulation within the abdomen‡. "There are," he indeed admits, "numerous instances of abdominal dropsies, in those labouring under a scirrhous or enlarged state of the liver; but there are, also, numerous examples of such states of the liver, as well as of the spleen and the other organs, without any such effusion; and in many cases, when such effusion has taken place, it has been carried off by the natural passages, or by tapping, without any return of the dropsy;—and yet without any visible change in the structural condition of the liver*."

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* I have seen, says Portal, in a man, in whom the parotid and maxillary glands of the right side were swollen and hardened, the whole of the right arm become œdematous. In another case, after an inflammatory swelling of the right parotid, there occurred an œdematous intumescence of the head and neck, at first only on the side with the inflamed parotid, but subsequently extending to the opposite side also;—the œdema passed gradually to the superior part of the chest, and from thence even to the abdomen. The inflamed gland finally suppurated and discharged a considerable quantity of purulent matter, after which the œdema quickly disappeared. *Op. citat.* Tom. I. p. 120.

† Anatomie Generale, Tom. I. p. 397.

‡ Vide Researches into the Nature and Treatment of Dropsy, &c. London, 1825

"I think," observes Dr Ayre, in another part of his work, "it may be assumed that ascites, when proceeding from visceral disease, (and the same principle applies to hydropic effusions from pressure of disease in other cavities) does so by the gradual extension of the chronic inflammation of the internal cellular or serous tissues of the diseased organ, to its outer or external coverings; and that, commencing here as from a point, the serous or hydropic inflammation is progressively propagated throughout the whole of the serous membrane of the cavity."

Although we are much pleased with the above explanation of the cause of dropsy subsequent to disease of the liver and other viscera, and believe that it will be found applicable to very many of those cases, yet we can by no means admit it to be true to the extent to which the author would wish to carry it. There is no doubt in our minds, that in not a few instances, one of the chief causes giving rise to dropsies, connected with certain diseased conditions of the liver, is an interruption to the free circulation through the abdominal veins;—the morbid condition of the organ, as discovered after death, allowing no room for the suspicion of a chronic or hydropic inflammation having been communicated from its substance to its peritoneal covering. In many cases of ascites it is evident that the blood circulates much more slowly, or in a much less quantity, through the liver, than under the ordinary circumstances of this viscus; it being found, after death, reduced to a hard, compact mass, from the vessels of which, on its being cut into, scarce a drop of blood escapes:—in other, and perhaps the greater number of cases, we have, however, an impediment to the circulation of a very different nature; there being congestion, with an overloaded state of the vessels of the liver, and of all the branches of the vena portæ, —a condition of things which, we are fully persuaded, whenever it is not speedily removed, occasions invariably a morbid effusion of serum, to a greater or less amount.

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* We hardly think this statement will be borne out by the general experience of physicians:—that the liver may be diseased without producing ascites we acknowledge; but when abdominal dropsy is connected with an organic affection of that organ, so long as this affection exists, we hazard the assertion, that no permanent cure of the dropsy can be effected.

Dr Ayre is forced, from the weight of facts, very reluctantly to admit that pressure upon the vessels of a limb will occasion an œdema below the spot at which the pressure exists.—"The remote cause is here," he remarks, "indeed of a mechanical kind, but not so the proximate cause of the effusion. By the resistance given to the blood's return by the principal veins of the limb, a reaction is occasioned in the extremities of the arteries leading into the corresponding extreme branches of the veins, which reaction is in this, as in a multitude of other occasions of congestive fulness in these vessels, a sanative effort of nature to overcome the primary obstruction." It is merely the fact of the obstruction, that we are now contending for;—to Dr Ayre's explanation of the mode in which the dropsy is produced in these cases we have no objection;—it is the one which we ourselves should advocate, if not in the exact form in which he has expressed it, at least in substance.

To the class of dropsies arising from diminished activity of the venous circulation, we refer, without hesitation, those infiltrations of the extremities consequent upon great prostration of the system. Take, for example, an individual during the period of convalescence from a violent febrile affection, or one who has recently experienced an excessive hemorrhage; no sooner does he rise from his bed and attempt the slightest exercise, than more or less of œdema will very generally occur in the feet and ancles. The venous blood, in such cases, accumulates in the vessels of the inferior extremities. In consequence of the languid and inefficient action of the heart and general circulation, and the prostration of the muscular system, it is not acted upon by a sufficient force to overcome entirely the effects of gravity which it experiences in the erect posture of the body. No sooner, however, has the action of the heart acquired its healthy vigour,—the muscles increased in tone,—and the patient enabled to deliver himself to a proper degree of exercise, than the intumescence of the feet and legs entirely disappears. It was a deficiency in the moving powers of the venous blood that produced it;—that deficiency being supplied, and the increased effusion upon which the œdema depended no longer continuing, the superabundance of serum accumulated in the cellular membrane is quickly removed by the absorbents, and the healthy condition of the parts is restored. But we must be cautious, not to confound with the partial dropsies just alluded to, those which occur subsequent to cases of violent disease, in which, as was stated in a former part of this essay, the morbid action has never been entirely subverted, but merely converted from an acute into a chronic form. Almost all the cases of dropsy collected by PORTAL, and supposed by him to have been occasioned by exhaustion from previous disease, are of this character, as is proved by a reference to post mortem examinations;—the facts revealed by which are strangely overlooked by the author.

Upon the above facts and arguments, we would lay down the two principal causes productive of dropsical effusion to be, 1st, Inflammation, either acute or chronic, of the serous or analogous tissues, and 2d, An impediment to the free circulation of the blood through the veins. Other causes of less frequent occurrence, or of a more equivocal character might be cited, but the space we have already occupied in a very brief and superficial examination of the facts adduced in reference to the two first, will prevent us, at present, from noticing the latter.

ARTICLE II.—*Observations on Pruritus Vulvæ. By* JOHN RUAN, M.D. *Fellow of the College of Physicians.*

[Read before the College, 27 May 1828.]

I have been so little in the habit of committing my thoughts to writing, that I should be wasting the time of the College, if I were to attempt a regular essay or dissertation,—but that I may comply with their requisition in the best manner I can, I will give a short statement of a few cases that have fallen under my notice, of a disease about which I believe very little has been published. I have been many years in practice, yet I never met with a serious case of pruritus vulvæ until the year 1822: the first case which engaged much of my attention occurred to me early in that year. The patient had been lately married, but did not become pregnant for some time after. She first complained of strangury, for which the usual remedies were resorted to, and at length was attacked with such an itching about the pudenda as made her miserable. Purging with various articles, diuretics, drinking largely of mild diluting drinks, with a free use of opiates and rest, procured some mitigation of her sufferings, but nothing like a cure was effected until the balsam copaiva was used. This, in doses of twenty drops three times a day, acted like a charm, and she was completely relieved. She has two or three times since had slight attacks of strangury, for which the balsam has been used with immediate advantage. I did not meet with another case of this kind until June 1825. In this instance the patient was far advanced in pregnancy, and the strangury was by no means an urgent symptom; but the itching was so intolerable, that although a delicate young woman, she could scarcely refrain from scratching in my presence. It did not occur to me to use the balsam, until I had tried bleeding, purging, diuretics and opiates freely and repeatedly, together with various applications to the part. She also washed the parts with lead water, and with borax dissolved in water; it was applied finely powdered. Lime water, warm milk and water, and poultices of bread and milk were tried. Indeed I may say, every thing I could think of, or that was suggested to me by some of my medical friends, was resorted to without any advantage. I had some how lost sight of the balsam; but at length it was brought to my recollection, and again answered perfectly. My patient was cured by it directly, and I now considered myself in possession of a very important, if not an infallible remedy for a distressing disease. But the very next case, which occurred in September 1826, showed the fallacy of this opinion. This woman, like the last, was near her confinement. The balsam failed entirely, but she was relieved by washing with a solution of borax. My fourth case was that of a lady about seven or eight weeks after delivery. Her confinement had been protracted for such a length of time by various complaints, and she was at last attacked by this very troublesome one, which set at nought all the remedies I had before made use of; but she was relieved by the carbonate of soda in small repeated doses. My fifth patient was delivered in July last; she was a woman of delicate health, suffered from a tedious labour, and did not receive all the attentions her situation called for; she had repeated attacks of fever during her confinement, and in the third week this distressing complaint came on with strangury, which called for the repeated use of the catheter. In addition to the remedies that had been tried in the preceding cases, an infusion of the common laurel was used as a wash, at the suggestion of a medical friend, but without effect. She could be kept in tolerable comfort only by the constant application of bread and milk poultices and large doses of laudanum. I now had recourse to the use of the lapis calaminaris, and had the part dusted with it, but she complained of this producing smarting and pain. Being desirous of trying it further, I mixed it with an equal quantity of powdered starch. She now bore it without complaining, and the itching gradually subsided. She continued the use of it for a week or two. This was the only case in which the parts were inspected by me. The swelling about the meatus urinarius was so great, that I was obliged to use a very small catheter, and the difficulty of passing this gave me a fair pretext for this examination, which discovered no other mark of disease than what might be attributed to the violent scratching.

Having given this short account of these cases, I refrain from any observation upon them, but shall be much gratified if I succeed in drawing information from my fellow members.

ARTICLE III.—*An Inquiry into the State of the Biliary Secretion in Fever.* *By* WILLIAM WOODSON WADDELL, M.D. *of Athens, Georgia.*

The arguments advanced in the following essay are founded on an opinion, opposed to the general belief, that the fevers of the southern states are bilious in their character. In proving this position, the author will be obliged to contend against popular prejudice, rather than medical authority properly so called; for it may be remarked, that it is the mass of the community who believe the doctrine, and not scientific physicians. Many professional men, no doubt, have fallen in with the prevailing opinion, that redundancy or depravity of bile exercises much influence in our fevers; but something has hitherto prevented them from accounting for or proving this position. In essays on fever, the subject is occasionally introduced, but, if we may judge from appearances, it has seemed to claim attention, only in consequence of the universal belief in the powerful agency of bile, and not from any conviction in the minds of the authors, that this agency was correctly imputed to it. It is not pretended, that the arguments here brought forward are altogether the result of observation; but they will not be condemned by the enlightened reader, because the proofs have not always been derived through the medium of the senses. Our senses are known to be imperfect; and in reasoning on subjects that require their minute employment in so great a degree as physiology and pathology, we arrive at a point where they totally fail to instruct. Here we draw on the resources of our reasoning powers; for it will be readily admitted, that many things are demonstrable to reason that are not to sense: reference to a principle in natural philosophy will convince us of the truth of this position. In endeavouring to detect the ultimate particles of matter by the aid of the microscope, they become so minute as to be confounded with the surface on which they are placed: at this point, the powers of the organ of vision assisted by the resources of art no longer avail; but reason goes many steps farther, and satisfies us that matter is infinitely divisible.

Hitherto, whenever the agency of the bile in fever has been doubted, the author of such opinion has been accused of mistaking cause for effect. To free the present essay from that imputation, three positions will be assumed.

1. That redundancy or depravity of bile is not the cause of fever.
2. That neither are the necessary effects of fever.
3. That neither are directly or indirectly the effect of the acknowledged causes of fever.

In considering the first position, we shall investigate the physiological effect of the usual quantity of healthy bile on the system, and thence form an opinion of the probable pathological influence of an increased quantity, or morbid quality of the same fluid. We are taught that it is the office of one portion of the bile to assist in the separation of the chyle from the chymous mass brought into the duodenum, and that the office of another portion is to stimulate the alimentary canal and cause it to propel its contents. The reality of the latter office is now a settled point in physiology. That bile contains a purgative principle has been proved by the experiments of professor CHAPMAN, who found that the bile of inferior animals was actively cathartic. Negative evidence to the same effect is derived from the fact, that in those diseases, where external symptoms indicate a deficiency of biliary secretion, there is always torpor of the intestines and obstinate costiveness. This is exemplified in jaundice, dyspepsia and several other diseases. Having these opposite effects according as the secretion is healthy or deficient, the natural inference would be, that a redundancy of the secretion would purge or produce diarrhœa. The same effects would result from acrimony or depravity of the bile. This state of the biliary secretion has been attributed to the influence of solar heat, occasioning an abstraction of the more watery parts which it contains. It is to be presumed that the bile, subjected to such a process, must acquire a concentrated power, and that its irritating and corrosive effect on the mucous membrane would be greater according to the intensity of the irritation. We should therefore expect either a violent diarrhœa, or a mild or severe dysentery. If then bile, in a state of redundancy or depravity, be the cause of our fevers, we have a right to expect that those fevers would be preceded or accompanied by diarrhœa or dysentery. But these affections, so far from being common, are unusual attendants on fever. Is it not, therefore, probable, that our fevers, called bilious, are in fact never caused by an altered state of the biliary secretion?

The second position assumed is, that redundancy or depravity of the bile is not the effect of fever. It would be transcending the prescribed limits of the present essay to attempt an exposition of the effects of all the phenomena of fever on the biliary organs. We may, however, take one of the most common, namely, universal oppression, followed by arterial reaction. It is agreed on all hands, that in the first mentioned state, there is a general diminution of secretion. It will also be conceded, that when reaction and arterial excitement take place, the secretory vessels will return to the execution of their proper functions in most of the glands. But the liver is known to be an exception to this rule, since it derives the materials of its secretion from its veins instead of its arteries. By the augmented action of the arteries, the veins become engorged with blood, and being destitute of any power of reaction, sufficient to overcome this morbid distention and obstruction, it follows that circulation will be partially, and secretion totally suspended.

But the most satisfactory view that can be taken of the subject, will be, to establish the third position assumed, or in other words to trace the common causes of fever in their influence on the animal economy, and ascertain whether they can be so modified in their application to the system, as to be productive of increased or depraved biliary secretion. This will in a great measure include all that it would be necessary to say in a separate consideration of the symptoms; for if we follow the causes to their ultimate effects, we shall certainly, at some stage of our progress, encounter the more prominent symptoms. Among the causes of fever, marsh effluvium presents itself as most important. The manner in which it enters the system is yet a contested point. By some it is believed, that it acts on the skin, that it paralyses the capillary vessels, and that the circulation, being thus obstructed on the surface, is greatly augmented in the deep seated vessels. The latter then make an effort to disburthen themselves of their load, but owing to their overpowered and debilitated state, they are unable to succeed, and the fruitless effort is called fever. Suppose we admit this theory to be true: the veins, and especially those that supply the larger viscera, would accordingly become engorged from two sources: they would receive their proportionate quantity of the blood confined to the internal parts by the paralysing effect of the miasmata on the surface, and they would have an unusual quantity forced into them by the increase of arterial action which always takes place in fever. As they are not capable, from their organization, of transmitting their contents in a ratio to be compared to the arteries, the engorgement will necessarily continue. Is it to be supposed then, that those veins, from which are derived materials for the secretion of bile, can in their disabled state furnish elements for the production of an unusual amount of that fluid. We are taught by physiologists, that the blood, before it arrives at a gland, must undergo some preparatory change, which predisposes it to furnish the elements for the peculiar secretion for which it is destined. But can this preparatory change take place, when the blood is forced so rapidly into the veins of the liver by the power of arterial excitement? And if it does not take place before the blood arrives at the place of secretion in the liver, can we expect it to occur after that time? In the latter case, the original ability of the blood to effect those changes, or its former susceptibility of having such changes produced in it, is most probably lost, because it has become morbid in character by having assumed an unnatural stationary position. It is necessary to the healthy condition of the blood that it should be decarbonized by the oxygen in the lungs; but this change cannot be effected in blood which is stopped in its course by engorgement in the vessels of a large gland, and thus prevented from passing through the lungs. Physiology recognises no other point at which the blood can come in contact with oxygen, and consequently carbon must remain in the engorged blood and thereby become diseased. It will of course no longer furnish the necessary pabulum for secretion.

A new theory of the modus operandi of miasma was published in Charleston in 1820. By this theory, it was supposed that it entered the system through the medium of the function of respiration; that being a combination of noxious gases, it acted only by virtue of its mass or bulk, by which, in a mechanical manner, it prevented the necessary quantity of oxygen from entering the lungs. That portion of carbon which is accumulated in the blood in the round of the circulation, being thus detained in it, was supposed to produce the same effect on the blood as so much milk, ether, alcohol, or any foreign substance, injected directly into the blood vessels. Now if this theory be true, the blood would be in a *positive* state of disease; but a healthy condition of the blood is requisite to supply the proper materials for secretion, and in its diseased state there will not be an increase but a diminution of this function.

It is believed by one class of pathologists, that miasmata affect the system through the medium of the olfactory nerves, and that their action is transmitted to the stomach by means of the intimate connexion existing between all parts of the mucous membrane. The nervous expansion of this tissue would, of course, be the principal agent in conveying the poisonous impression. But can we suppose that the nerves, in the performance of this office, will remain free from disease themselves? Can they be the messengers which carry death and disorder to every other part of the system, and yet be unaffected in their own delicate structure? Certainly not; for if marsh miasma has any existence, it must diminish, and possibly paralyse, the energy of the nerves when applied to them. It is a beautiful doctrine in physiology which teaches us, that secretion depends for its immediate cause on an intuitive perception of the nerves, by which they discover that the materials for secretion are passing along in the blood vessels and are ready to be drawn out. As soon as this perception is made and communicated to the proximate secerning agents, they immediately act and the work of secretion takes place. Rejecting the idea of any mechanical agency in a process so purely vital, and assuming the nervous influence as the efficient cause of secretion, it becomes evident that any interruption of that influence must cause a corresponding derangement in the action of the secreting power of the glands. Now according to this theory of the modus operandi of miasmata, the energy of that part of the nervous system, which is subservient to the secretion of bile, is directly diminished (for the choledic duct and glandular acini of the liver are lined with a continuation of the same tissue on which the miasmata first act), and therefore secretion from this gland will be suspended and not increased.

At first sight, this view of the case will appear directly opposed to the law laid down by BICHAT, that irritation, being caused at the mouth of a duct, will be transmitted to the gland from whence such duct proceeds, and will give rise to secretion. But it yet remains to be proved, that the action produced by miasma is properly speaking irritation. The fact that chemistry has not yet detected its existence gives reason to believe, that the effects usually attributed to it are owing to the abstraction from the air of something which constitutes its wholesome stimulus rather than to the presence of an unwholesome principle capable of producing irritation.

Inasmuch as it is presumed that the action of miasmata on the nervous expansion of the mucous tissue tends to deprive that portion of the nervous system of its inherent power of discriminating the proper materials for secretion in the blood vessels; as moreover those materials are necessarily allowed to remain in the circulation; and as we know from analysis that the constituents of bile are very unfavourable to health;—may not their detention in the vena portarum be the cause of some of the anomalous phenomena of fever yet unaccounted for? The presence of bile in the blood has been denied; but its elements may certainly exist, and not be cognizable to our senses. The adoption of these views is not to be considered as implying a belief in the doctrines of humoral pathology. There is no primary disease of the fluids alleged, but a disordered condition only of those fluids, the consequence of a morbid action first developed in the solids. The following quotation from the writings of Dr SAMUEL JACKSON will support this view of the subject. "Is it not probable," says he, "that, in protracted diseases, attended with interruption of secretion and excretion, digestion and healthy nutrition, the quality of the blood may become depraved to a certain extent, so as to become incapable of a healthy restoration to irritability?"

The opinion is adopted by professor CHAPMAN, that miasmata, as a cause of disease, become entangled in the food and saliva, and thus enter the stomach. It would create but a slight modification of disease, whether the primary impression were made on the stomach, or any of the arbitrary anatomical divisions of the intestines. The mucous tissue, which lines the whole, is most immediately exposed, and no doubt bears the brunt of the attack of the morbid agent. But it has been shown that miasma acting on this membrane cannot be supposed to cause an increased secretion of bile.

Some experiments have been made, intended to prove that miasmata are taken into the system by pulmonary and cuticular absorption. The authors of the most conclusive of these experiments tell us, that the proof of the presence of the deleterious matter in the system, was derived from its discovery in the blood and some of the excretions. Under these circumstances (allowing the theory to be correct), it must have affected the circulatory mass directly, in the manner already alluded to; and it has been contended, that we could not rationally expect increased secretion of bile during the existence of such a state of things. The general conclusion may be drawn, then, from what has been brought forward, that marsh effluvium, as a cause of fever, cannot, from any mode of its application, either directly or indirectly, influence the portal vessels to excessive elaboration of bile.

There are other causes said to produce fever, and among them solar heat is often mentioned. The ultimate effect of this agent on the system is atony of the extreme vessels, resulting from their previous great excitement, and, as a consequence of the former, the destruction of the balance of the circulation. In this state of things, it is the venous system alone which suffers, and it has been shown, that, when the veins are engorged, we cannot rationally expect undue secretion of bile. But there is also a negative proof that heat has not this effect; for if we attribute such a result to it, we virtually ascribe to it the influence of a universally pervading cause. In a country like that of Carolina or Georgia, where the heats of summer are oppressive, we should always be afflicted with those diseases (diarrhœa, dysentery, &c.) which result from redundancy of bile. But this is not the case. There are many places where diseases solely attributable to peculiarity of climate are not known. There are positions proverbially healthy; and so great is their celebrity, that they are thronged every summer with the ranks of wealth and fashion flying from the miasmatic districts of St Mary's, Darien, Savannah, Charleston, &c. The places alluded to are not attended with any of the collateral advantages of mineral springs, &c. but are attractive merely on account of their salubrity.

The agency of heat in the production of increased biliary secretion may be looked upon almost as a settled doctrine in pathology. Amongst scientific men, the opinion owes much of its popularity to the support it derives from the celebrated JOHNSON, whose works are so much read in the United States. In the present essay, it has not been questioned, that heat may vitiate the bile, by causing an absorption of the serosities; but sufficient reason for denying the existence of that process in our febrile diseases has already been stated; namely, the absence of the first symptoms (diarrhœa, dysentery, &c.) that would most probably follow on that supposition.

Let us next inquire, whether solar heat has any peculiar power of generating a redundancy of bile. Dr JOHNSON's views are founded on a belief in the existence of a sympathy between the vessels of the surface and those of the liver, whereby an impression made upon the former will be propagated to the latter. Hence he supposed, that the application of heat to the surface, and consequent increase of perspiration, would be productive, sympathetically, of an increase of bile.

In studying the character of this theory, the following axioms in physiology should be kept in view: 1. The blood affords the pabulum for all the secretions and excretions. 2. There is but a definite quantity of this fluid in the animal economy; and from it, as a grand reservoir, all the secretory glands and depurating emunctories abstract or receive a part for their own proper purposes. 3. When, on the application of stimulus, any one of those glands or emunctories is excited to increased action, there must be a proportionate diminution of action in every other; but more especially in those connected with the excited organ by sympathy. The first of these axioms will not be disputed. If our second position be true, that there is but a definite quantity of the material of secretion, and if we suppose the already large portion of this material carried off by the skin to be augmented, may we not presume, as a necessary consequence, that every other gland and emunctory will secrete or carry off less? But if the third axiom be true, shall we not, for a still stronger reason, be entitled to infer, that increase of perspiration will cause decrease of bile? It is admitted on all hands, that increase of urine will cause decrease of perspiration, and vice versa;

but, according to Dr **Johnson**'s theory, we are not to be content with supposing that the diminution of perspiration in diabetes mellitus is sufficient to balance the increase of urine, but must also suppose deficiency of bile in the same disease. Do reason and observation warrant us in believing that the quantity of urine secreted and carried off by the urinary apparatus is equal to the sum of the cutaneous depurations and hepatic secretions? A quotation from **Bichat** may render the general position here assumed more tenable. "Heat," says he, "which expands the cutaneous system, increases its action at the expense of that of the glands, and reciprocally, cold, which contracts it, forces the glandular system to supply this action."

A very prominent objection, drawn from every day's observation, may be urged against Dr **Johnson**'s theory. If it be an invariable law of our economy, that there shall be an increase of bile as a consequence of increase of perspiration, why is it that labourers, who are generally bathed in sweat, are free from those diseases which result from increase of bile? To account for the absence of diarrhœa in these cases, we are told by Dr **Johnson**, that nature establishes a vicarious sympathy in the mucous membrane, by which it is caused to secrete less mucus, and the intestines, being consequently destitute of their natural lubricity, do not so readily yield to the purgative influence of the bile. But if they do not yield to the *purgative*, will they not, when deprived of their mucous defence, more readily suffer under the *corrosive* influence of this "secretion when in a depraved condition?" Contrast Dr **Johnson**'s opinion on the subject with that of Dr **Cleghorn**. "Sharp, indigested humours, flowing from the stomach, or, which is more frequently the case, an increased secretion of bile and other liquids, necessary for chylification, produce a simple looseness, which gradually washes off the mucus from the intestines, erodes their membranes and at last brings on severe gripes with bloody stools." This paucity of mucus, which Dr **Johnson** thinks necessary to prevent diarrhœa, will, according to Dr **Cleghorn**, cause dysentery in its worst form. But labourers are not particularly subject to either disease. Therefore the theory is improbable. Dr **Johnson** refers to some experiments of **Bichat**, which prove that during the time of digestion in the stomach the biliary secretion is diminished; but that as soon as the food passes the pylorus it is increased. He traces the same analogy here, asserting that in the first part of the process the skin is dry and hot, but when the food gets into the duodenum, the surface relaxes, and perspiration is much increased. Suppose the fact perfectly established, that the liver pours its bile into the duodenum immediately on the passage of the chyme through the pylorus, how are we to detect the time so precisely as to be able to say that there is an increase of perspiration at the same moment? We cannot tell from mere sensation. For since digestion is a process under the influence of the great ganglionic nerve, the sensorium does not take cognizance of it, and no one but the dyspeptic, whose stomach is in a pathological state, is ever sensible of the passage of food through the pylorus.

To enumerate all the causes of fever, and explain their modus operandi, would be but to repeat the main topics of the argument already presented. The uniformity of the symptoms of our fevers proves, that cold, and moisture, or the two combined, as well as acrid ingesta, all produce the same effects as miasmata and solar heat; the most prominent of which is the destruction of the balance of the circulation. And it has been proved that such a state cannot be reasonably supposed to produce a redundancy or depravity of the biliary secretion.

Inasmuch then as these states of the biliary secretion do not obtain, either as the cause or effect of fever, nor yet as the indirect result of any of the usual causes of fever, they may be assumed to have no connexion with febrile disease whatever. This supposed connexion between bile and fever has been copied by one physician from another, until its belief has been sanctified by age, and it is now deemed heterodox to depart from it, merely from the force of early impressions.

There are some signs which are usually considered diagnostic of a bilious habit, but which can be easily shown to have no connexion with it whatever. It is said, that in our fevers, we have dark, tar-like, or parti-coloured stools, which are known to consist of vitiated bile. In regard to these stools, let us inquire whether they are the necessary and natural attendants of fever? Are they ever seen previous to the exhibition of calomel? May not calomel by its energy (which we are told is principally expended on the liver) cause the flow of an unusual quantity of bile? But if it be asked whether calomel is capable of inducing so vitiated a condition of the bile, the answer is, that it probably can. This is a point which cannot be determined by experiment, and must therefore always remain problematical.

We may procure pure and healthy bile and genuine calomel; we may mix them and apply a temperature equal to that of the alimentary canal, but yet observation made on such a mixture would throw no light on the reciprocal action of bile and calomel in the system. We might add to the mass, the gastric and pancreatic juices, and suppose the peristaltic motion and the action of the different viscera to be severally unimportant to perfect the process, still there would be wanting an indefinite something, to render our deductions legitimate. Chemistry, with all the light it has thrown on modern science, cannot conduct us into the hidden recesses of the system, nor show us the modus operandi of the thousand agents concerned in the functions of the animal economy. It is certainly rational to suppose, that the vitiated appearances in the stools of febrile persons who have taken calomel may be the result of some unexplained combination of the fluids of the alimentary canal with bile and that medicine. Substitute any other medicine, and we shall still be in doubt whether a combination of the same kind may not take place.

A case however may be supposed, wherein the state of the excretions above alluded to may exist, independently of the exhibition of any purgative medicine. Dr **Daniell**, of Savannah, tells us that the evacuations which followed the application of sinapisms to the abdomen were very offensive. To account for this, without the necessity of supposing the bile to be primarily predominant or vitiated, it is necessary only to refer to the true state of the biliary secretion in our fevers. That it is diminished is proved by the constipation always present; and it is proved negatively by every argument in the former part of this essay. But better authority can be given. "Contrary," says professor **Chapman**, "to the prevalent notion, in the fevers emphatically termed bilious, there is a great deficiency of bile; sometimes, the liver being so torpid by the force of the remote cause, or by early congestion, that its secretions are suspended, and, until their restoration, we advance slowly with the cure." Now, on the application of a sinapism, the portal congestion is relieved, the circulation is equalized, and the blood, from which the elements of the bile are derived, being restored to comparative health, the secretion will return in increased quantity, proportioned to the time of suspension. From detention in an unnatural situation, it is probable also that it will have acquired a vitiated appearance and offensive character.

Yellowness of the eyes is looked upon as one of the signs of a bilious habit. The colour is often said to exist, when it cannot be perceived, unless, forsooth, the physician takes the trouble to press down the lower eyelid, when a yellow tinge can be seen, which in a great majority of instances is apparent in perfect health. Dr **Fordyce** suggests, that this is owing to a redundancy of the sebaceous matter, natural to the part. Dr **Daniell**, on the contrary, thinks it the result of a diseased state of the capillaries. But let us admit that it is the result of bile; then so far from this appearance indicating that the biliary secretion is excessive, it merely indicates that the usual quantity is obstructed in its passage into the duodenum by some pathological state of the ductus communis, or the gall-duct proper. It is the cystic bile (if it be bile at all) which is diffused over the system, whether it be by absorption or regurgitation. But the manner in which the appearance is explained by Dr **Daniell** is infinitely more satisfactory.

The fact has been much relied on, that, as a precursor of our common autumnal fevers, we have a vomiting of bilious matter. This does not prove, however, that there is any derangement deeply implicating the biliary secretion. Among those causes that produce fever, there are some whose primary effect is to cause nausea and vomiting. This nauseating cause may be so extended as to invert the peristaltic motion in the duodenum; hence the choledic duct may be emulged, and healthy bile, being brought into the stomach, will in most cases be ejected. If it be more decided in colour, or bitter in taste, than natural hepatic bile, we might readily explain these qualities, by supposing the gall bladder (which, owing to the contraction of the abdominal muscles in vomiting, is pressed upon by the surrounding viscera) to have evacuated its contents. If we were called upon to give a correct name to the fluid thus vomited, it would be wrong to call it bilious matter, even though we had reference to natural bile only. Dr **Fordyce** suggests, on this subject, that we are sensible of the presence of bile, because it is rendered conspicuous by its characteristic properties of bitter taste and smell, and decided colour; while the gastric and pancreatic fluids (being no doubt also present) are deficient in similar striking properties. The choledic duct opens into the duodenum, and were bile superabundant or acrid, it is more probable that it would produce its natural effect (diarrhœa or dysentery), than that it should by sympathy nauseate and cause vomiting. One result we can explain, by referring it to actual contact and direct action; the other can only be accounted for through the medium of the more mysterious agency of sympathy.

It may be objected to this paper, that even if the position assumed in it were true, it answers no practical end; or in other words, that though redundancy or depravity of bile have no connexion with fever, yet deficiency of the same fluid may have great agency in the disease. Accordingly it may be said that the term bilious and the mercurial practice will be equally proper on both suppositions. The term however is objectionable, because it will uniformly convey the idea of superabundance of bile. In regard to the unity of the practice in deficiency, redundancy, or depravity of the bile, the enlightened physician need scarcely be reminded, that, although the same medicine may be used in all these cases, yet the mode of administration in each is quite different, and followed by widely opposite results. In the case of redundancy of bile, supposing it to be too bland to carry itself off, the maximum dose of calomel is given, followed by laxative medicine; thus extending its influence so far only as to cleanse the primæ viæ and carry off the offending matter: but in deficiency of bile, arising from torpor or congestion of the liver, the calomel is given in small and repeated doses, in order to secure a stimulant operation on the liver, by causing its effects to accumulate in the system in a gentle and gradual manner. The secretions are thus solicited and not forced; and the patient convalesces without that afflicting and permanent derangement of the digestive functions so often the consequence of the exhibition of large doses of purgative medicine. That this derangement is often the sequel of a mercurial treatment, we are the more easily persuaded to believe, when we reflect on the cruel and unnecessary waste of the gastric, pancreatic and biliary fluids which it occasions,—the high grade of excitement to which the organs which furnish those fluids are necessarily subjected,—and the subsequent debility and collapse which they must suffer. This practice with mercury especially demands reform in South Carolina and Georgia, where calomel is so exclusively relied on in fever.

ARTICLE IV.—*Observations on the Injurious Effects of the Stimulating Treatment applied to Articular Tumours which appear to have passed to the Cold or Chronic State, but which nevertheless require a continuation of Antiphlogistics—or a soothing method. Addressed to the Kappa Lambda Society of Philadelphia. By* M. CHAUFFARD, *Principal Physician of the Hospitals at Avignon, Vaucluse. Translated by* D. THEODORE COXE, M.D.

[We are indebted to Dr LA ROCHE, one of the Editors of this Journal, who is now in Europe, for forwarding the two following very interesting papers. As many of our readers do not read the French language, we have deemed it best to publish a translation of them, and we are indebted for the English dress to Dr D. THEODORE COXE.]

It is generally admitted that inflammatory diseases long treated by emollients finish by requiring the use of tonics; the more plausibly in proportion as the tissues are less susceptible, less porous, less permeable, less accessible to the sanguineous engorgement. Thus, in most works and most consultations, it is recommended to treat by excitants, after a longer or shorter period, according to the individual, all those tumours which are developed around the articulations, and which seem to have their especial seat in the periosteum, the aponeurotic extremities of the muscles, the ligaments, the fibro-cartilages, and the dense cellular tissue interposed between all these.

This method, which is willingly extended to all cases, sometimes however fails of success: and its obstinate application keeps up, on the contrary, in certain circumstances, the morbid accidents; increasing them, and not always being innocent of the irremediable alterations of texture which bring with them the loss of limb or life.

On this subject I offer some observations.

In 1816 M. C. 20 years of age, made a false step, and immediately there ensued a considerable engorgement of the whole left tibio-astragalian joint. Very proper means were immediately put in practice; first refrigerants, then cataplasms and local bleedings. The inflammatory fluxion was of the most intense kind, and a month after the accident there remained much swelling and pain upon the slightest motion: nevertheless, the skin was pale and rather shining, as though erysipelatous, being depressed with ease, and retaining for a long time the impression of the finger. The individual was habitually pale, of a phlegmatic temperament. Tonics were judged necessary, and the moment of administering them appeared to have arrived. Under their influence, the tumour enlarged, dull pains were more constantly felt, the patient became paler, the complexion wan, and the organic functions languishing. The swelling extended to the whole lower extremity, and the health declined—a stronger reason for insisting upon the treatment which seems so appropriate to such a state of general and local weakness. M. C. departed for the waters of Grevulx, but found no amelioration there; on his return he wished for no more remedies and confined himself to keeping the parts warm and in the most absolute repose. A cure took place, but very gradually. This patient could not walk for a year, and then used crutches for more than six months.

False ideas, resulting from the aspect of the skin, the continuance of the swelling, the existence of a lymphatic temperament, make us forget that the phlegmasiæ of the articulations, proceeding from a violent shock, are seldom dissipated, but with extreme slowness; thence is derived a stimulating system of treatment, entirely opposed to the nature of the disease. The sad consequences are referred to the weakness of the individual, possessed as we are with the idea, that after a given period excitants are demanded. In addition, it is not for a moment suspected, that the languor of the functions may, with the other symptoms, depend, at least in part, upon the tonic treatment so unhappily substituted for the emollient. Without the abrupt determination of our patient, who can say he would not have died, or that the softening of the whole articulation would not have necessitated the amputation of the limb?

Mademoiselle Gasp. 14 years old, having menstruated for some months, her neck studded with suppurating scrophulous ulcers, was attacked in 1819, with a considerable engorgement of the left knee. She had a very white skin, considerable fat, the upper extremities pretty large, and the lower extremities very small. The engorgement of the knee was developed like a white swelling; it became hard, little sensible, and so large that the leg could not be bent upon the thigh. Internally, she took the martial preparations, wine, strong meats, the cruciferæ, sulphur, bark, syrup of Portal, elixir of Peyrylhe: externally, were applied fumigations, aromatic bags, camphorated brandy, stimulating frictions, alkaline baths; for a month douches, at first alkalized, afterwards sulphurous. Under the influence of all these means, which appeared however so strongly indicated, there was no amelioration, but extreme emaciation of the limb, sensible increase of the tumour, manifest decay, the ulcers of the neck deep and spreading, with ill-conditioned pus. We at last changed this treatment, thinking we could perceive an obscure fluctuating movement upon the lateral posterior parts of the tumour. A large emollient cataplasm was applied entirely round the knee, and she took white meats, eggs, panada, turtle and snail soup, milk, and white decoction. These mild measures being perseveringly continued, the health was gradually restored, the ulcers of the neck cicatrized, and the whole mass of cellular lardaceous tissue, engorged with lymph or serous fluid, which constituted the tumour of the knee, was converted into pus; fluctuation being sufficiently evident, an opening by caustic was made in the sac, and issue given to a quantity of pus of a pretty good quality. The openings gradually healed, and the cure, though very slow, was complete, constantly under the influence of the same regimen, and of local emollients. The girl now enjoys excellent health, uses her leg freely, and walks as if the limb had never been diseased.

The conclusions to be drawn from this case are so evident, that it is almost useless to enumerate them. In short, no morbid state seemed better adapted for stimulants, and nevertheless, they were constantly useless, and kept up a sort of hectic fever, contributing to the emaciation, probably through the medium of a chronic gastroenteritis, of which they were the source: they operated disadvantageously upon the progress of the ulcers, and the scrophulous tumour. Very different results and a cure followed purely analeptic and emollient measures.

In 1821, Bl. 12 years old, was brought to me from a neighbouring village, pale, thin and suffering from a considerable tumour around the external malleolus of the left leg, and a pretty large wound upon the external lateral face of the foot, at the inferior part of this tumour. These were all consequent to a fall the child had had from a cart, in which this foot was much bruised. At the commencement of the disease, emollients were used; but as the cure was not as prompt as was expected, recourse was had to topical excitants, with a view of resolving this engorgement. From this time the disease increased, and œdema gradually seized upon the whole leg. An ulcer was then formed, and, in spite of ointments, continually spread, furnished a serous pus, and remained tender and wan. I advised simple dressing with pledgets of lint, surrounding it with a fine linen bandage smeared with cerate, and covering the whole joint with a poultice of flaxseed, or bread-crumbs boiled in a decoction of marshmallows; to keep the limb also perfectly quiet, to give the child goat's milk three times a day, with barley water sweetened; and to nourish him with fecula, eggs, and other analogous food of easy digestion. Four months after commencing this treatment, the health was restored, the wound healed, and the tumour almost entirely gone.

Had the *pretended resolvents* been continued, this engorgement would certainly have degenerated into a white swelling, perhaps incurable. The best books, too, lean to this erroneous determination. "The use of these topicals [emollients]," says M. Boyer, "is continued as long as the pain and inflammatory engorgement exist; but as soon as they disappear, recourse should be had to resolvents, such as aromatic wine, camphorated brandy, &c.*" Precepts too absolutely expressed, since in similar circumstances to the one in question, *though there may be neither pain nor inflammatory engorgement*, there still remains an organic susceptibility which these measures heighten sufficiently to revive serious accidents, to perpetuate a dull phlegmasia, &c. Besides, since the most absolute rest is recommended, even long after the success of the treatment, *for fear of keeping up irritation and inflammation*, p. 13, why advise measures which so often lead to this result? It appears to me a contradiction.

Subsequently to an acute articular rheumatism with which Mademoiselle Cré was affected, continual pains remained, situated principally over the right instep. Thence there resulted a gradual displacement of the os scaphoides which exposed the head of the astragalus, and at the same time a well marked incurvation of the foot outwards; in addition, there were pain and habitual engorgement of this extremity, which exercise and even standing increased. In this state, in April 1825, the patient, whom I had not yet attended, went to Montpelier, to consult M. Delpech. She was 17 years old, menses regular, in good health although of a lymphatic temperament; and yet, notwithstanding these propitious circumstances, this able practitioner was fearful lest a white swelling should be formed in the midst of the spongy bones of the tarsus. He consequently ordered strict rest in bed, temperate regimen, light food, such as white meats, vegetables and river fish; saponaceous injections, purgative and tonic pills daily, as long as they did not irritate the bowels, and produce more than one or two stools in the 24 hours. Every eight days, leeches in the neighbourhood of the deformity; twice a day, for an hour, a local bath of a decoction of the leaves of solanum dulcamara, followed by an embrocation of oil of hyoscyamus, and then a poultice of rye meal and flaxseed; subsequently and upon the visible decline of the disease, caustic potash was applied between the tibia and tendo Achillis, two inches above the line of the internal malleolus: the treatment concluded with the use of the waters of Mont d'Or.

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* Mal. Chirurg. Tom. IV. p. 12.

The chief aim in these directions was, a temperate regimen, complete rest of the part, frequent local bleedings and topical emollients. It seemed however so natural, considering the date of the disease, the nature of its cause, the presumable degeneration of an acute into a chronic rheumatism, and the *atonic* swelling of these soft bones, to prescribe dissolvents, resolvents, topical excitants and douches of strong mineral waters, natural or artificial! Nothing was joined to this simple and luminous plan but revulsives to the intestines, and they scarcely influenced the cure; for the patient being incommoded by them, I suspended them several times according to the spirit of the directions which had been confided to me. As to the cautery, she was as it were well when it was used, and M. Delpech, who again saw her at this time, rightly judging it useless, discontinued it. The cure was already so complete, that the journey to the waters of Mont d'Or caused neither displacement nor swelling, and was merely followed by œdema of the subcutaneous cellular tissue, which vanished after two days' rest.

Mademoiselle Cré now stands nearly all day behind a counter, and has even laid aside her boots which were lined on the inside with a plate of steel to sustain a part once so much deranged.

In the summer of 1825, young Matilda B., returning with her father from Bourg to Lyons, rested all night on her left wrist, her hand being shut. Torpor and pain in this joint were the result; and eight or ten days after, the motions of this hand became difficult, and she could no longer play on the piano. Compresses dipped in brandy were applied round the joint, and the patient continued to use the hand as well as she could, making great exertions. The wrist gradually swelled, and at length all motion was lost, the hand seeming to be lengthened and always hanging. Her parents then quitted the country and went to Lyons. The most careful attention was paid her by Mess. BOUCHET and BAUMERS, and an antiphlogistic and emollient local treatment adopted. The patient, though thin, and slightly developed for her age, enjoyed excellent health. She persevered for some time in the same plan, but no amendment took place. The wrist being much deformed and swelled, with no redness of the skin, and very little pain, these gentlemen supposed the really inflammatory period of the affection had passed, and tried slightly stimulating douches, the application of aromatic bags, and then a pretty tight bandage of flannel, surrounded by oiled silk*. Instead of diminishing under these new measures, the tumour still increased, without however showing any external sign of cutaneous phlegmasia. Matilda was sent to pass the winter in the south, that the functions of the skin being there favoured by the influence of the sun, her disease should not at least be aggravated, if this extensive revulsion should continue inefficacious. I was entrusted with the care of the patient, and for some days continued the above mentioned bandage, carefully examining the tumour. Whether a more decidedly inflammatory action now began, or whether the fatigue of the journey excited its development, I soon perceived the necessity of returning to the original treatment. Leeches on several parts of the tumour, baths and emollient cataplasms were applied, and a slight diminution of the engorgement resulted; but the skin reddened at the palmar surface of the joint, pus collected and finally escaped by a spontaneous opening; it was serous and ill conditioned. This opening enlarged, and a ragged ulcer succeeded. The same treatment was continued, and a new collection of pus was formed at the right side of the joint, and afterwards on the left: this necessitated the application of caustic potash. The suppuration of these wounds progressively amended, and at the end of two months, the hand was less pendent, the tumour much smaller, and the wrist not so deformed. The resolution of the tumour however was far from being complete, and the ulcers did not heal. I then tried, still continuing the same local treatment, to give more activity to the absorbents of the limb by friction, with half a drachm of strong mercurial ointment, upon the arm and forearm. No effect being appreciable, I communicated the facts to my learned brethren of Lyons. Their opinion was, that the active movement of the tumour must have ceased, that the articular engorgement was taking on the chronic character, and consequently, that the relaxing treatment had better be laid aside, and local baths applied, composed of a solution of Flemish glue and hydro-sulphuretted liquor. Light douches upon the joint, of the same mixture, at a suitable temperature; bags with a mixture of powdered henbane, chamomile, elder and carbonate of magnesia; frictions upon the engorged parts with hydriodate of potash ointment; and a flannel compress bandage, were also recommended. In the meanwhile, professor LALLEMAND, who was passing through Avignon, saw the patient, and gave a similar opinion. He thought that local alkaline baths, general baths, with a decoction of sage, lavender, rosemary, and an addition of two glasses of brandy should be used; and afterwards sea bathing, &c. I acquiesced in this unanimity of sentiment, proceeding from such enlightened men, and thenceforward used baths slightly alkalized and general stimulating baths alternately, with the bandage, intending subsequently to use the hydro-sulphuretted douches, the aromatic bags, &c. Ten days had not passed away when the tumour, instead of diminishing, seemed to increase, pains were felt, and the little ulcers began to bleed. We had finally to return to the same emollient measures, which calmed this momentary excitement, brought things back to their former state, and at length completely cured the patient. When she returned to Lyons, the ulcers were cicatrized, the wrist was very little deformed, and the hand could be moved pretty freely. I now merely applied a roll bandage, and convalescence continuing, the mother living in the country, nothing more was done.

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* The arm had been, from the commencement of the treatment, in a sling; the hand supported by a splint, and consequently immoveable.

In this case again certainly, the cessation of the emollients, and the application of tonic means seemed indicated in every way. When however they were employed, bad consequences soon followed.

I do not report these facts with the intention of absolutely proscribing, from the treatment of these articular affections, the use of tonics (I am not ignorant that cases exist where they are followed by success); but solely to show, that at a period, improperly determined by theoretic ideas, there is danger in prescribing them indiscriminately, and that in many circumstances a milder method can alone accomplish a cure.

Of the five cases I have reported, none is furnished by an adult; and the most obstinate disease is that of the eldest of the individuals mentioned, which confirms, in my opinion, this axiom of M. Boyer:

"In general, and under ordinary circumstances, white swellings are more serious in feeble and cachectic subjects than in the well made and healthy, and in young people and adults than in children." *Op. cit. p.* 507.

ARTICLE V.—*Removal of the Penis, by means of Dr* MAYOR'S *Constrictor, or Ligature Tourniquet. By* M. FRANCOIS A. BINET, *Surgeon of the Hospital De la Croix at Nice, Piedmont. Communicated for this Journal. Translated from the M.S. by* D. THEODORE COXE, M.D.

M. B. seventy-two years old, of a strong constitution, bilious and sanguine temperament, having never been seriously ill, had entered very young into the navy. He was married at thirty, and in a few years had four children, one of whom is still living. At the end of twenty-three years of wedded life, his wife died of disease of the breast, and after three years of widowhood he again married; his second wife is still living, enjoys excellent health, and has made him the father of five children, all equally robust and healthy.

During the life of his first wife M. B. after a journey perceived that his prepuce became gradually contracted; as he did nothing for it, in six months he had a phymosis. This state, the cause of which M. B. never could imagine, was present when he remarried.

Some years after, the emission of urine having become very difficult from the almost cartilaginous induration of the opening of the prepuce, an inconvenient itching between this and the glans was felt, which increased so much, that M. B. was almost constantly forced, even during sleep, to scratch it violently with his hands, which produced a secretion, and flow of very fetid lymphatic fluid.

M. B. now consulted a surgeon, who thought he saw in this fetid secretion, and in the phymosis, an old syphilis, and treated him accordingly*.

A vast number of mercurial pills, frictions of mercury along the penis, VANSWIETEN's liquor, and many other remedies were employed, but without any amelioration; on the contrary, the penis increased in size and became painful. Two respectable surgeons of this place, being consulted, unknown to each other, disapproved of the medical treatment M. B. had undergone, and advised baths, emollients, and a treatment capable of arresting the evil and preventing the cancerous degeneration of the penis which might have been the consequence.

M. B. paid no attention to this wise advice, but preferred using the universal remedy, LE ROY's purgative, and took about nine bottles of the different numbers of this drastic in the space of five months; he then passed to the use of many internal and topical remedies, which quacks extolled as infallible, but all in vain: the penis became more and more painful, and of such a size, that the circumference of its extremity was nearly seven inches; the skin which covered this part was stretched, shining and livid; and the secretion from the prepuce was greatly augmented.

The weight of the penis and the pain prodigiously increased, and fever and emaciation soon reduced him so low that he could no longer quit his bed. He was in this miserable state when another quack undertook him.

Topicals of every description were employed by this last, but in less than a month several holes appeared in the prepuce, which gradually extended along the penis even to its root. Fungous excrescences which were infinitely multiplied, appeared across these holes, and in a few months the penis was entirely covered by them.

M. B., alarmed at the progress of the disease, called me in, (May 1827) and I found him in the following state: extreme paleness; great emaciation; a frequent, small, feeble, extremely irregular, and often even intermittent pulse: insupportable pain day and night in the penis, the end of which was as large as the fist; prepuce and glans so changed as not to be recognised; the whole penis was a mere mass of fungous excrescences of a cancerous nature, like a decayed cauliflower, studded with livid points, and hollowed out by deep fissures, whence the urine flowed as from a watering-pot; an abundant ichorous and very fœtid suppuration assailed the nose at a distance. Round the root of the penis, in a radius of ten or twelve lines, and an inch and a half towards the scrotum, the skin was livid and hard, and full of little varicose blood-vessels, and callosities or tubercles of the size, nearly, of hemp-seed. The right inguinal glands were very little engorged—the left on the contrary very much so; two of them were as large as common nuts. The patient had kept his bed for about ten months.

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* M. B constantly assured me, that neither he, nor his two wives had ever been affected with any syphilitic disease.

My first care was to endeavour to moderate the pains, which gave him no respite; and to restore his strength by food of easy digestion: the penis was dressed with lint, dipped in a tepid solution of sedum minus and carrots, to which were added, at each dressing, a few drops of cherry laurel water. In twenty days the patient gained strength; the pains were mitigated; the swelling of the inguinal glands nearly subsided; and the fungosities of the penis so far diminished, that for one while I thought they would entirely disappear. These appearances however being but deceptive, and the disease continuing to progress towards the root of the penis, I concluded that, to save the life of the patient, recourse must be had to amputation. This operation was more promising, inasmuch as the disease of M. B. did not present the characters of a cancerous diathesis; neither did it depend upon a syphilitic affection, but solely on a very old, accidental phymosis*.

I then desired two distinguished surgeons, old friends of M. B. to visit him with me. They agreed with me as to the propriety of the operation, and that the moment was still favourable for attempting it. We visited him a second time two days after, to examine more carefully the state of the urethra, and the root of the penis. The urethra, examined in the perineal region, presented no callosity, irregularity, nor augmented sensibility.

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* The authors ROUX and WILLIAM KEY report a great number of cases of amputation of the penis for cancerous affections. Most of those operated on, who were radically cured, were advanced in life, had not been affected with syphilis, and the development of the cancerous affection had been preceded by a phymosis dependent on some constitutional or accidental but always old defect.

Among the fistulous erosions, whence, as I have said, the urine flowed, we observed two, placed laterally at the root of the penis, much larger than the others; into these I had before introduced and kept up for two days a pretty thick dossil of lint, the better to ascertain their direction and depth. Examined by the blunt probe, we found they were lined throughout with the same fungous excrescences as the penis, and penetrated the lower part of the symphysis pubis to where the urethra passes over the bifurcation of the corpus cavernosum. Should we then amputate the penis in the ordinary way, or was it better to try Dr Mayor's mode *?

I was strongly inclined to the latter, as it was not at all prudent to cut off the penis far under the symphysis pubis; the operation would have been tedious, painful and very difficult; and the hemorrhage, in spite of every possible care to stop it, might have had bad consequences, particularly in a patient weakened by age and suffering, and whose pulse was small, irregular, and often intermittent. I therefore determined to procure the ligature tourniquet, and in the meanwhile, the patient continued the treatment adopted, with the addition only of prepared sponge, by means of which I succeeded in a few days in considerably enlarging the two lateral fistulæ at the root of the penis, and in more fully ascertaining the direct communication which existed between them and the urethra, beyond the bifurcation of the corpus cavernosum. The 24th of September last, at eight A. M., provided with a constrictor, and assisted by two very intelligent young surgeons, I operated in the following manner.

I cut the skin around the root of the penis, where it ceased to be livid, hard and tuberculous; I then hastened to isolate this member beyond the root, by dividing with the fingers the cellular tissue which here surrounded it, and with the bistoury some aponeurotic fibres, and its suspensory ligament. Four small arteries were cut in this operation, but as I hastened to tie them, the hemorrhage was trifling. I then placed the constrictor as deeply as possible, i. e. upon the bifurcation of the corpus cavernosum, near where the urethra was opened by the lateral fistulæ of the root of the penis†, which facilitated my

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* Dr Mayor's method, the success of which has astonished many operators, is intended to remove by ligature certain tumours and degenerated organs, I am not aware, however, that any other has up to the present time employed it for the removal of the penis.

† I did not think it neçessary to attempt passing, with the constrictor, the part where these fistulæ terminated in the urethra, which would have been painful

drawing the penis in its horizontal direction; I then made a transverse incision upon the inferior surface of this last, about four lines on this side of the constrictor, to discover the urethra, into which I introduced a very short silver catheter, which I pushed about an inch beyond the constrictor: this I made gradually act, until the patient experienced a supportable sense of constriction, proposing to tighten it the next day.

The operation being thus finished, I covered the whole with a simple dressing. For half an hour M. B. complained of a pretty sharp pain, without being able to assure himself it was the effect of the constriction of the penis; but he afterwards conversed very cooly and tranquilly, complaining of no pain: the urine flowed through the catheter. About 2 o'clock in the afternoon, he had a severe chill which lasted until four, when warmth returned. At five I found him moist, with no pain in the part operated on; the fungosities of the penis discoloured and faded; and the pulse still small, very frequent, and soft without intermission.

I directed one of the young surgeons, who was to be with the patient during the night, to relax the constrictor, in case he perceived its action likely to cause spasms or very acute pain. I told him, it was the more necessary to be careful in this respect, as the constrictor being deeply placed and on an erectile organ provided with many nerves, and endowed with great sensibility, it might cause serious morbid phenomena.

The 25th, I learnt in the morning that the patient had been agitated by a burning heat till midnight, and did not sleep till towards 3 o'clock, after having soaked four shirts with perspiration. I was at the same time agreeably surprized at the report from the young surgeon respecting the constrictor, which he had just removed, observing, that the penis was in a state of complete mortification. A few hours afterwards, I called to see M. B. He was very calm, complaining of no pain; his pulse was less frequent than the evening before, but still small, and without intermission. The penis exhaled an intolerable stench, and was putrid. I took away with the scissors all on this side the opening by which the canula penetrated the urethra; lotions of tepid water were applied this and

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and difficult; besides I was persuaded the action of the constrictor would cause mortification in a line or two of the penis beyond the point upon which it acted: I also foresaw that the internal sides of the above mentioned fistulæ could not escape the action of the constrictor.

the four following days, at the end of which time, the rest of the penis, which was mortified by the constrictor, separated entirely*. The wound then assumed a very fine colour, and was dressed with cerate. The metallic catheter was changed for a gum elastic one.

As, when I operated, I did not spare the skin, and was obliged to cut it to within an inch around the root of the penis, and even more on the side of the scrotum, a considerable space of an ovoid figure was left between the lips of the wound, whose larger end was towards the linea alba, and the smaller towards the scrotum; this space was about three inches in the centre, and five and a half from one end to the other. This however I did not mind, as M. B. having, in an advanced age, passed from a state of embonpoint to one of emaciation, the skin was pretty ample; it could readily slide upon the subjacent layers, which permitted the approximation of the edges of the wound, by means of simple adhesive strips which I used with the greatest possible success.

M. B. quitted his bed on the 20th of October. On the 2d of November he began to acquire strength and flesh. The wound is now closed; the skin is cicatrized in the form of a funnel an inch in diameter, at the bottom of which the opening of the urethra is perceived, which is about an inch and a half below the surface of the skin. He urinates with much ease, the water being often propelled nearly a foot; and the scrotum has become entirely pendent and of a round figure.

Reflections.—Writers upon the operations of surgery make no mention of the amputation of the penis, at the part where I applied the constrictor, in M. B.'s case; the principal motive for which silence I conceive to be, that in a similar case, after the operation, the divided arterial vessels would be very deeply hidden, and consequently too difficult to be seized upon for arresting the effusion of blood.

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* The removal of the penis by the constrictor did not cause acute suffering to M. B., as he experienced after the operation, and that only for half an hour, a vague sensation of pain, which he was not even sure was occasioned by the constrictor. As further proof of this, the morning after the operation M. B. asked me, if it would be long before I amputated the yard, an operation he dreaded on account of the pain; when told it had already been done since the last evening, he wished to be sure of it himself, unable to believe that the excision of a part so sensible as the penis had caused him so little pain.

Dr **Mayor**'s method being the only one capable of obviating so great an inconvenience, the profession doubtless owes him great acknowledgments; and I must certainly say, to his praise, that if I had had no knowledge of his constrictor, I should never have attempted the operation upon M. B., and he would inevitably have perished by the progress of his disease, and amidst great sufferings.

Article VI.—*History of a Remarkable Case of Obstruction of the Bowels which ended in Recovery. By* Franklin Bache, M.D. *Physician to the Philadelphia Prison. Read before the Kappa Lambda Society of Philadelphia,* 23 *April* 1828.

Matilda Bayard, a stout, short mulatto, aged 15 years, was admitted into the hospital of the Philadelphia prison on the 27th of January 1827. She complained of pain over the abdomen, and her bowels had not been moved for several days. Ordered five grains of calomel, to be repeated every four hours until the bowels are moved.

Jan. 29th. Bowels not having yielded, and dysuria being present,—omitted the calomel, and ordered a mixture of sweet spirit of nitre and sulphate of magnesia, to be assisted by injections of the latter salt. The pain over the bowels having increased, ordered twenty cups to the abdomen.

31st. Bowels not moved. Directed the warm bath, and the mixture of sulphate of magnesia and sweet spirit of nitre, and the injections to be continued.

Feb. 1st. No alvine evacuation yet obtained. Omitted the above remedies, and ordered half a grain of the extract of elaterium to be given every hour, until the bowels should yield, to be assisted by injections, containing spirit of turpentine.

2d. No stool. Omit elaterium, and resume the mixture of sulphate of magnesia and sweet spirit of nitre, and saline injections.

Feb. 7th. Between 2d and 7th, the sulphate of magnesia mixture and injections were the principal remedies employed, and the bowels were slightly opened. This day, the patient appearing to be worn out with pain and much exhausted, ordered calomel gr. iii. with opium gr. ½, three times a day, and a nourishing diet, consisting of beef tea, chicken broth, and arrow root and milk.

9th. Bowels not open, and dysuria continues. Omitted the calomel, and ordered the compound tincture of aloes, and uva ursi.

12th. Since the 9th, has taken the compound tincture of aloes, and uva ursi. The bowels, however, have not been properly opened. This day my friend, Dr THOMAS HARRIS, saw the patient with me, and recommended a combination of calomel and opium. Accordingly, ten grains of calomel and one grain of opium were directed to be taken three times a day. At the same time, thirty drops of the spirit of turpentine were ordered, to be repeated at the same intervals, with a view to stimulate the kidneys and bowels.

13th. No stool having been obtained, the calomel was discontinued, and four half-grain pills of elaterium ordered, to be given at intervals of two hours.

14th. The bowels still refuse to yield. Ordered twenty cups to the abdomen, fomentations, sulphate of magnesia, and injections.

16th. No motion yet obtained. Ordered twenty cups to the small of the back, half-grain doses of elaterium every two hours, and the fomentations to be continued.

17th. Discontinued elaterium: ordered cream of tartar and jalap, which was continued on the 18th.

19th. Since the 17th, the bowels have been opened, but not completely. I now determined to bring the patient fully under the influence of mercury, if possible. Accordingly five grains of calomel and one grain of opium were directed three times a day, and frictions with mercurial ointment.

21st. The bowels becoming again closed, ordered cream of tartar and jalap every other day, and the calomel and opium to be continued on the intermediate days.

23d. Ordered injections, to be repeated daily. The pudendum having become inflamed and swelled, directed a poultice of bread and milk.

March 1st. Since the 23d ult. the cream of tartar and jalap, and the calomel and opium have been continued on alternate days, as well as the injections, but without producing a stool or affecting the mouth. Omitted these remedies, and resorted again to the elaterium in half-grain doses during this day. The poultice was discontinued.

March 5th. The patient being worn out with pain and fatigue, and despairing of getting her bowels opened, ordered two grains of opium at bed-time.

9th. Complains of soreness of the mouth. Ordered two ounces of sulphate of magnesia, and the opium to be diminished to a grain and a half.

12th. The bowels have not been moved, and all the symptoms becoming aggravated, such as violent pain over the bowels, with distention of the abdomen,—ordered twenty cups to the abdomen, a powder of cream of tartar and jalap every two hours during the day, and the opium to be continued at bed-time.

13th. Ordered the warm bath.

14th. Sulphate of magnesia mixture and injections were tried, but without success. On the 16th, the elaterium was resorted to again.

17th. Twenty cups were directed to the abdomen, and a blister to be applied immediately afterwards.

19th. The elaterium was discontinued, and the sulphate of magnesia mixture resumed.

21st. The elaterium was resumed, three half-grain pills being given.

22d. Much pain existing in the loins, and there being an almost constant suppression or retention of urine, I this day formed two caustic issues in the small of the back, in the hope that they might produce a salutary impression on the kidneys and bowels by counter-irritation, or the discharge they might create. The opium at night was increased to four grains.

24th. The bowels have yielded to a certain extent, and the patient appearing to be weak, ordered a pint of wine and half a pound of mutton daily.

25th. The patient appearing to be sinking, I ordered the volatile alkali, beef tea, and half a pint of brandy, instead of the wine. Reduced the opium to three grains at night.

April 1st. Since the 25th ult. the remedies have been elaterium, (on the 27th), sulphate of magnesia, injections, and opium every night. This day, the symptoms becoming again formidable from the obstruction, and the danger appearing more imminent from the state of the bowels than from the existing debility, ordered the elaterium once more, to be assisted by fomentations and sulphate of magnesia. Continued opium at night.

April 3d. Ordered thirty dry cups to the abdomen. Continued the sulphate of magnesia, and directed injections.

4th. Discontinued the brandy, and ordered half a grain of elaterium every two hours. Continue opium at night.

In the foregoing account of this case, the general character of the symptoms has been recorded from memory, or inferred from the remedies employed, of which a register was kept from day to day. From this time, however, memoranda of the principal symptoms were made at the bed-side.

5th. This day, had a violent vomiting, and threw up fæces. Ordered injections of sulphate of magnesia, to be repeated every two hours.

6th. Feels no pain in the abdomen, unless when considerable pressure is made. Directed powders of cream of tartar and jalap, and the enemata to be continued.

7th. No evacuation per anum having taken place, the cream of tartar and jalap were omitted, and twenty grains of calomel ordered, with a continuation of the enemata. The stomach being sick, directed the effervescing powders.

8th. No evacuation. Repeated the calomel, and continued the effervescing powders, and enemata.

9th. Vomited fæces. Since the 4th, three grains of opium were taken pretty regularly at bed-time.

10th. Resumed the use of the cream of tartar and jalap, and discontinued the enemata, the patient being fatigued and rendered sore by them.

11th. Examined the caustic issues, and finding them disposed to heal, discontinued the poultice, and ordered the savine ointment. The bowels continuing still obstructed, and the abdomen being distended and painful,—ordered twenty dry cups over that part, and the elaterium to be resumed, to be assisted by cream of tartar and jalap.

13th. Yesterday, took four half-grain pills of elaterium, and two powders of cream of tartar and jalap, without producing a motion. Ordered an injection, containing ten grains of tartarized antimony.

14th. The injection of tartar emetic produced a copious stool yesterday; and the abdomen, which before was very much distended, became considerably less immediately afterwards. Notwithstanding this relief, the patient vomited fæces to-day. Stop the cream of tartar and jalap, continue elaterium, and repeat the injection of tartar emetic.

April 16th. No stool since the 13th. Continue elaterium and resume the injections of sulphate of magnesia. Foment the abdomen.

17th. The symptoms derived from the obstruction being all aggravated, I discontinued the elaterium and opium at night, and ordered twenty-four cups to the abdomen, the warm bath, two ounces of metallic mercury, and injections of warm water.

18th. The injections yesterday succeeded. Eleven syringefuls of warm water, amounting to about two quarts, were thrown up, and immediately produced a large stool, containing three or four scybala, about the size of an unshelled walnut. After an interval of a quarter of an hour, eleven syringefuls more were injected, and caused a smaller stool, containing two scybala, about the size of a hickory nut. The injection being repeated a third time, was followed by a third stool, consisting of liquid fæces.

19th. The bowels having yielded, I determined once more to attempt to bring the patient under the influence of mercury. Accordingly I ordered the blue pill, and frictions with mercurial ointment.

20th. Had a stool, in which a portion of the metallic mercury, given on the 17th, was observed.

23d. From the beginning of her indisposition, the patient has had an almost constant difficulty in making water. Sometimes there is a retention, at other times a suppression of urine. For several weeks together, no urine appeared to be secreted. Within the last four or five days, however, the secretion has been restored, but the urine has required to be drawn off by the catheter.

24th. The opium was resumed, in two-grain doses at bed-time.

27th. Although the injections of warm water have been occasionally used since the 20th, the bowels have not been opened since that date. Ordered jalap ʒi. cream of tartar ʒii. to be given immediately, and the blue pill, frictions, and injections to be continued.

May 6th. Since the 27th of April, the mercury has been continued, assisted by sulphate of magnesia, without opening the bowels. During the same interval, the effervescing powders and sweet spirit of nitre have also been frequently resorted to; the former for sick stomach, the latter as a diuretic. The patient appearing to be sinking, ordered half a pint of brandy, to be taken daily in the form of milk punch.

May 7th. Discontinued the mercury, having failed to affect the mouth. The debility appearing to be greater, increased the brandy to a pint, and ordered volatile alkali. The bowels still remain closed.

13th. Since the 7th, palliatives, stimulants, and nourishing food have been the remedies; such as opium, sulphuric ether, brandy, and beef tea. Since the 8th, the opium has been increased to five grains at bed-time. This day, ordered injections, assafœtida mixture, and frictions with spirit of turpentine, and discontinued the volatile alkali and sulphuric ether.

16th. Reduced the brandy to half a pint and discontinued the beef tea. Examined the caustic issues, and found them reduced to the size of a quarter of a dollar. Continue the dressing with savine ointment.

17th. This day, examined the os tincæ and rectum and was unable to detect any unnatural structure. The bowels remaining still closed, and the secretion of urine suppressed,—ordered the compound tincture of aloes, a table spoonful three times a day, injections, and sweet spirit of nitre.

20th. Discontinued the brandy.

23d. No evacuation from the bowels since the 20th of April. Ordered injections, to be repeated every afternoon. Continue the compound tincture of aloes, sweet spirit of nitre, and the five grains of opium at night.

25th. Bowels still closed. Omit compound tincture of aloes: give ten grains of calomel, three times a day, every other day, and a diuretic mixture of sulphate of magnesia ʒi. and sweet spirit of nitre ʒii. on the alternate days.

June 4th. No stool. Continue the calomel, and the diuretic mixture mentioned on the 25th ult.

8th. Discontinued the allowance of milk.

15th. Mouth not sore—no stool. Continue calomel and diuretic mixture on alternate days, and use mercurial frictions. Reduce opium to four grains at bed time.

18th. Vomited fæces, with half a pint of liquid. Continue calomel and diuretic mixture. Reduce opium to three grains at night.

19th. Vomited fæces last night and this morning. Reduce opium to two grains at night.

22d. Vomited fæces and liquid.

June 28th. Suspect the calomel has not been taken regularly, the patient having a repugnance to being salivated. Directed the calomel, diuretic mixture, and mercurial frictions to be continued. Omit the opium altogether. The catamenia began to flow yesterday.

30th. Yesterday vomited a small quantity of fæces after the visit.

July 2d. Despairing to affect the mouth, I this day discontinued the calomel and mercurial ointment. Continue the diuretic mixture.

4th. Omit the diuretic mixture, and give a powder of cream of tartar and jalap every hour until the bowels are moved.

7th. Continue the cream of tartar and jalap, and assist its operation by injections of warm water. Ordered six cups to the head, and sweet spirit of nitre.

8th. Omit the cream of tartar and jalap. Continue the sweet spirit of nitre. Give thirty grains of calomel, and tincture of assafœtida, a teaspoonful every hour.

9th. The bowels have not yielded. Stop sweet spirit of nitre and tincture of assafœtida: give spirit of turpentine, a table spoonful every two hours.

10th. Discontinued the spirit of turpentine. Ordered injections of salt and water. Resume the allowance of milk.

11th. Ordered large injections of water.

12th. Vomited fæces mixed with liquid, about a quart. Continue injections.

13th. Last night, vomited fæces mixed with liquid. Ordered effervescing powders.

15th. Ordered thirty grains of calomel, and continued the injections.

16th. Directed fomentations over the abdomen, and a powder of cream of tartar ʒi. and jalap ʒss. to be taken every two hours. Continue injections.

17th. Yesterday, vomited fæces, mixed with liquid, and had two stools, one large, the other small, being the first evacuations per anum since the 20th of April. Continue fomentations and injections of warm water. Apply ten cups to abdomen. Give ten drops of laudanum and two teaspoonfuls of sweet spirit of nitre every hour. Resume the opium, in five grain doses at bed-time.

18th. Had a stool last night, in which something, resembling shreds of membrane, was floating. Ordered half a grain of extract of elaterium every hour. Continue the laudanum mixture, directed yesterday, and the five grains of opium at night.

July 19th. Had another stool, containing membranous shreds as before. Vomited fæces, mixed with liquid, several times yesterday and last night. Ordered effervescing powders. Omit elaterium.

20th. Vomited fæces this morning, and several quarts of liquid having a urinous smell. Feels no sensation of water in the bladder, but complains of pain in the bowels. No stool to day. Continue the sweet spirit of nitre, without laudanum. Discontinue the opium by the mouth, and use it as a suppository. Give injections of brine.

21st. Vomited about a pint of fæces and liquid. Continue injections. Stop opiate suppository for one night.

22d. No stool since the 19th. Ordered one drop of croton oil every hour.

23d. Vomited fæces last evening and this morning. Repeat effervescing powders.

24th. Vomited fæces last night and this morning. No evacuation per anum. Ordered five drops of croton oil, to be taken in an ounce of mucilage.

26th. Last night vomited fæces and liquid.

27th. Vomited fæces yesterday afternoon. Continue sweet spirit of nitre, effervescing powders, and opiate suppository.

28th. Vomited last night fæces and liquid, with some blood. Ordered injections, in addition to the remedies mentioned yesterday.

29th. Vomited fæces and liquid this morning.

30th. Vomited liquid and some blood this morning. Catamenia began to flow yesterday. Ordered one drop of croton oil every hour, to be followed the next day by injections.

August 7th. Vomited fæces on the 2d, 3d, 4th, and this date, the remedies mentioned on the 28th ult. having been continued.

9th. Last night succeeded in obtaining stools by injections; the first since the 19th of July.

11th. Vomited fæces and liquid this morning.

12th. Explored the rectum by the finger, and by means of a large catheter, but had no evidence of obstruction.

15th. Was bled to the extent of ten ounces for convulsions. Continue sweet spirit of nitre, effervescing powders, and opiate suppository.

Aug. 16th. Vomited, at two different times; first hard black fæces, and then liquid fæces. Ordered hydrocyanic acid.

18th. Last night vomited fæces, so consistent as to cause the patient great difficulty in forcing them up.

19th. Last night vomited thin liquid fæces.

20th. Vomited thin liquid, coloured with fæcal matter. Milk discontinued. Ordered one pigeon daily, for soup.

21st. Vomited fæces, with liquid, and some blood.

23d. Vomited last night about two quarts of fæces and liquid. Continue sweet spirit of nitre and effervescing powders, and reduce opiate suppository to three grains.

25th. Vomited fæces last night and this morning.

26th. Stercoraceous vomiting recurred.

27th. Reduced the opiate suppository to one grain, at night.

28th. Vomited thin liquid last night and this morning. Ordered fifteen cups to abdomen.

30th. No evacuations per anum since the 9th. Ordered one drop of croton oil every hour, as long as the stomach would bear the medicine, or until the bowels are moved.

31st. Bowels yielded yesterday and this morning; quantity of fæces and liquid discharged per anum estimated at two gallons. Notwithstanding this relief, the stercoraceous vomiting recurred to-day.

September 1st. There being great pain in the abdomen, ordered fifteen cups, and fomentations to the part.

3d. Vomited hard fæces last evening, a quart or more. Continue sweet spirit of nitre, effervescing powders, and opiate suppository.

4th. Increased opiate suppository at bed time to four grains.

5th. Vomited yesterday afternoon four quarts of fæces and liquid. Ordered three drops of croton oil.

7th. Vomited fæces last night and this morning. Continue croton oil.

8th. Took yesterday, and last night, twelve drops of croton oil without producing vomiting or purging. The stomach, however, is very sick.

9th. Last night, after a very severe pain, the patient, while on the close stool, voided something suddenly from the bowels,—its nature not ascertained. Ordered twenty grains of sulphate of zinc, as an emetic, and directed half a pint of brandy and a pint of milk daily.

Sept. 10th. Vomited half a pint of yellow liquid, of the consistence of pus, after the emetic. The directions of yesterday having been misunderstood, ten cups were applied over the abdomen after the operation of the emetic. The abdomen is softer this morning. The lips are blue, and there exists great anxiety in breathing. Catamenia commenced flowing to-day. Ordered the volatile alkali, and reduced the opiate suppository to three grains. Continue sweet spirit of nitre, and omit effervescing powders.

13th. Within a few days past the patient is incapable of speaking or protruding the tongue, although she appears sensible of what is passing around her. Vomited blood and watery liquid last night. Ordered half a pound of mutton daily, instead of the pigeon, for soup. Continue opiate suppository, volatile alkali, and sweet spirit of nitre.

16th. Passed a restless night. Ordered tincture of iodine, twenty drops, three times a day. Continue sweet spirit of nitre and opiate suppository. Omit volatile alkali.

17th. Ordered injections of a solution of sulphate of magnesia.

18th. After two injections were administered the patient was sick at the stomach. Castor oil was then given, and she vomited two quarts of fæces and liquid. Ordered ol. ricini and injections to be continued daily.

19th. Took half a pint of castor oil yesterday, and used injections. Had the bowels open for the first time since the 31st of August, the stools being thin and watery. Notwithstanding, the stercoraceous vomiting recurred in the afternoon. Continue tincture of iodine, sweet spirit of nitre, and injections. Increase opiate suppository to four grains; omit castor oil.

22d. On the 20th, 21st, and this date, vomited fæces, from half a pint to a pint and a half.

23d. Ordered fomentations, and substituted tobacco injections for those prepared with sulphate of magnesia, one drachm of tobacco to a pint of boiling water, half to be thrown up at a time.

24th. Half an hour after the administration of the tobacco injection yesterday, became very sick at stomach and vomited a pint of fæces.

25th. Continue tobacco injections.

26th. Reduce opiate suppository to three grains.

Sept. 27th. Yesterday, after taking the tobacco injections, vomited about a quart of fæces and liquid. Omit tobacco injections.

29th. Last night vomited about a quart of fæces.

30th. Vomited yesterday afternoon a pint of fæces. Discontinue brandy ordered on the 9th.

October 2d. Vomited fæces yesterday and to-day, about a pint each time.

5th. Vomited fæces and liquid, about a pint.

6th. Rested well last night, although the suppository of opium was not given. Vomited this morning one quart of fæces and liquid.

7th. Vomited last evening somewhat more than a quart of fæces and liquid. The catamenia began to flow yesterday.

8th. Vomited last evening about a quart of fæces and liquid. Has used the tincture of iodine and sweet spirit of nitre at irregular intervals since the 19th ult. also the opiate suppository, with the exception of the night of the 6th. Resume the injections of sulphate of magnesia.

11th. Vomited fæces and liquid on the 9th, 10th, and this date.

14th. I was present to-day for the first time while the patient was vomiting fæces. They were so consistent as to proceed from the mouth in the usual form of healthy evacuations per anum. The patient laboured dreadfully, and appeared almost choked. About this time the tincture of iodine was omitted.

16th. Vomited more than a pint of liquid fæces yesterday afternoon. Between eight and nine o'clock yesterday evening the patient was seized with a violent pain and bearing down. Two syringefuls of water were thrown up into the bowels, and were followed by a hard black stool. Then thirteen syringefuls were administered, and produced a thin yellow stool. Continue the sweet spirit of nitre and suppository; also the injections, to be aided by a pint of castor oil, to be taken in two doses.

17th. Had yesterday morning, after the visit, a stool, consisting of broken-up fæces, about a pint: in the afternoon, another, about half a pint, and thin. Continue sweet spirit of nitre and suppository; also ol. ricini and injections.

18th. Had a small thin stool.

20th. Had a stool last night after the injections.

21st. Had two stools last night and one this morning. About this date, the sweet spirit of nitre was finally discontinued.

24th. Had a stool last evening and one this morning. Discontinue the ol. ricini and substitute the sulphate of magnesia, two ounces daily. Continue suppository, and omit injections.

Oct. 26th. Omit sulphate of magnesia: resume ol. ricini daily. The patient begins to sit up.

29th. Had a stool consisting of broken-up fæces. The suppository of opium, which had been continued in various doses at bedtime since the 20th of July, was finally laid aside on this date. Continue ol. ricini.

30th. Complains of pain in the side. Ordered ten cups to the part.

31st. No stool since the 24th. Ordered, instead of the castor oil, half a grain of elaterium every two hours, until the bowels are moved.

November 2d. Stop elaterium; resume ol. ricini.

4th. Stop ol. ricini; give sulphate of magnesia three ounces.

5th. Had two stools last night, and one this morning.

7th. Took sulphate of magnesia yesterday and to-day in two-ounce doses, and has had four stools since last visit. Gains strength rapidly.

9th. The sulphate of magnesia was repeated.

10th. The allowances of mutton and milk were discontinued, and the patient was put upon the regular hospital diet, consisting of chocolate and tea, with bread for breakfast and supper, and bread for dinner.

14th. Since the 10th, the patient has improved rapidly, and was this day discharged cured.

There are two circumstances connected with this case which I have deferred to mention to this place, in order to avoid tedious repetitions. I allude to the state of the urinary organs, and the occurrence of convulsions. In the narrative of the case, under the date of the 23d of April, the condition of the urinary secretion up to that time is referred to. From that period until the 7th of June there was no evidence of the secretion of urine. During the same interval, no evacuation occurred from the bowels either by vomiting or purging; so that it is not probable that the nurse was deceived. On the 7th of June, the secretion of the urine was restored; but it was retained in the bladder, and required to be drawn off almost every day until the 16th of July, when it appeared to be once more suppressed. From this date, until the 25th inclusive, the secretion was almost entirely suppressed. During this interval, the catheter was used frequently, either without effect, or with the result merely of obtaining a table spoonful or two of urine. During this period, stools and stercoraceous vomitings occurred; so that the evidence of the suppression of the urine is not so complete, as on the former occasion. Within this same interval, namely on the 20th of July, it will be recollected that the patient vomited several quarts of liquid having a urinous smell, just such an occurrence as might be expected, during a continued suppression of the urinary secretion.

From the 26th of July until the 5th of September, the catheter was used almost every day, and from a table spoonful to a pint of urine drawn off at each operation. From the 6th to the 11th of September the secretion was more abundant, a pint of urine being drawn off each day. It may be worthy of remark, that it was within this interval (on the 9th) that the patient voided something suddenly from the bowels, the discharge of which may have had an influence in increasing the action of the kidneys.

From the 12th of September until the 26th of October, the catheter was used daily, with the exception of three days, and from half a gill to a pint of urine drawn off. On the 27th of October one pint of urine was passed naturally. After that date, the urine was again suppressed for a short period; but on the 5th of November, it once more passed naturally, and has not since been either retained or suppressed. The sweet spirit of nitre, so constantly exhibited during the progress of the case, was directed to the relief of the urinary organs.

The disorder and abolition of the urinary secretion, which were such prominent symptoms in this case, ought not to excite surprise, when taken in connexion with the other remarkable features which it presented. A priori, we might be led to suppose that a long continued suppression of the alvine discharges would cause a diminution, if not suppression, of the secretion of urine, and facts are not wanting which prove this position.

The other circumstance in this case, alluded to above, the mention of which has been deferred, is the occurrence of convulsions. They first occurred on the 26th of May, and finally disappeared on the 15th of October. During this period, the patient was affected with twenty-six distinct attacks, consisting variously of from one to fourteen separate convulsions. These convulsions were often very formidable, and so sudden in their access as frequently to throw the patient out of bed: they were attended usually with long periods of insensibility between the paroxysms.

On reviewing the foregoing case, it will be observed that from the beginning of the attack, on the 27th of January 1827, until the 5th of April, when the stercoraceous vomitings first occurred, the bowels yielded several times imperfectly; namely about the 5th of February, between the 16th and 19th of the same month, and on the 24th of March. Taking into consideration all the circumstances, I think these small discharges per anum proceeded from below the obstruction, being probably feculencies washed out by the frequent enemata employed. On the 13th of April, the first stool was obtained, subsequent to the occurrence of the stercoraceous vomiting, after an interval of obstruction of twenty days counting from the 24th of March, or of seventy-six days reckoning from the commencement of the disease. Stools were then obtained on the 17th and 20th of April. The next occurrence of stools was on the 16th, 18th and 19th of July, after an interval of eighty-seven days. On the 9th of August, stools were again obtained—interval twenty-one days. On the 30th and 31st of August, stools—interval, twenty-one days. On the 19th of September, stools—interval, nineteen days. On the 15th of October, stools—interval, twenty-six days. From this time the alvine discharges took place regularly.

During the intervals above mentioned, the stercoraceous vomitings occurred pretty regularly, and formed a kind of substitute for evacuations per anum. Indeed, the relief derived from the fæcal vomitings was so evident, that on several occasions, when, from a burning sensation in the stomach, the patient was satisfied that the fæces were in that viscus, I gave remedies to promote vomiting. It was with this view that I prescribed the sulphate of zinc on the 20th of September. There was one period, however, of considerable duration, during which no evacuation of fæces either by the mouth or bowels took place. This was the case from the 20th of April until the 18th of June, a period of fifty-eight days. During a large portion of this interval also, namely, from the 23d of April until the 7th of June (44 days), no urine was secreted.

The diet of the patient during her long illness was very various; being sometimes very low, at other times cordial and nourishing, according as symptoms of an inflammatory character or of exhaustion were most predominant. There were periods of long abstinence during the worst state of the stomach from nausea and vomiting, and also intervals of calm, during which the patient ate with considerable appetite. One character of the case was extremely striking, namely, the absence of emaciation. The patient had indeed lost flesh and the integuments of the extremities were loose and flabby; but there was no attenuation of the limbs, such as is frequently seen in phthisis, and other chronic diseases. I attributed the absence of emaciation to the energy of absorption from the bowels, created by nature's efforts to relieve herself from the load caused by the obstruction.

In casting a glance over the circumstances of this case, and the different therapeutical measures resorted to, it may be profitable to inquire what remedies were most efficient in relieving and finally overcoming the obstruction of the bowels. This inquiry presents many difficulties; as we cannot be certain whether the bowels yielded to the medicines employed, or to the weight and pressure of the fæcal accumulations. The copious stool, obtained on the 13th of April, immediately followed the administration of an enema of tartarized antimony; but the whole effect cannot be attributed to this measure, as elaterium, and cream of tartar and jalap were freely given on the preceding days. The stools produced on the 17th and 20th of April appeared to depend on the administration of the metallic mercury. Next occurred the longest interval of obstruction, reaching to the middle of July, when the evacuations occurred, containing shreds of membranous matter. No doubt these were portions of the internal coat of the intestines, which had separated by sloughing. Still however the obstruction was not removed. The discharges per anum, mentioned as remarkably copious, occurred on the 30th and 31st of August, after repeated doses of croton oil, and may be supposed to have given the greatest relief; and on the 9th of September, the expulsion of something suddenly from the bowels took place. I regret exceedingly that the nature of this substance was not ascertained; but may we not conjecture that it was a portion of intussuscepted intestine, which had been detached by sloughing? Assuming this conjecture to be probable, it still is difficult to explain why the obstruction did not yield from this moment. Nevertheless the obstruction recurred. On the 19th it again yielded to injections and castor oil, and was finally and permanently relieved by the same remedies on the 15th of October. Upon the whole, I should be inclined to believe that injections of warm water, to act by its *quantity*, and the croton and castor oils were in this case the most efficacious remedies.

Since the occurrence of the above case, I have been desirous of ascertaining whether any analogous one is on record; but my time has not permitted me to make the necessary researches. I have however, in my reading, recently met with several cases possessing some points of resemblance. In the Dublin Hospital Reports, Vol. IV. 1827, Dr Crampton relates several cases of long continued constipation, the principal one of which has several characters in common with the case detailed in the present communication. In Dr Crampton's principal case, that of a female, there occurred frequent vomitings; the matter ejected being sometimes stercoraceous, and sometimes urinous both in taste and smell, while but little urine was "either secreted into, or passed from the bladder." The patient vomited every day; the intervals between the occurrence of stools became longer, and the urine was seldom passed. In this case, like mine, the catamenia occurred. In 1827, the patient had been in this state for *seven years*, and for the last eight months of this time "had no passage from her bowels, and only two or three during the preceding year."

Dr Crampton next relates the case of Anne Free, in which "both the bowel and urinary discharges were nearly suppressed." She never passed urine, except when relieved by the catheter;" and was affected with stercoraceous and urinous vomitings. This patient lived for several years.

In the Bulletin des Sciences Medicales for March 1828 there is related a case taken from a German periodical, as drawn up by Dr Berres, professor of pathological anatomy at Lemberg, which tends to show the connexion between obstinate constipation and the suppression of the urinary secretion. The subject of this case was a boy, thirteen years of age. On the 22d of October 1822 he was seized with fever, which, with another attack occurring during convalescence from the first, left him in a state of great debility. He had an obstinate constipation which lasted for ten or twelve days. On the 20th of November the patient was seized with violent pains in the rectum, which were much aggravated by efforts at stool. In January 1823, the constipation continuing, the urine was observed to be diminished, and its secretion became suppressed for fifteen days. Two similar suppressions occurred between this period and March, when a third took place, and continued for six months.

In July 1823, the patient was in an advanced state of marasmus. The constipation continued, and he was affected with a periodical pain in the loins, of such violence as to cause him to lose all sense and to become affected with convulsions. The belly was distended and elastic, and the bladder appeared to be empty. After the lapse of six weeks from this period, the urine began to flow abundantly, in consequence of a paroxysm of fear, brought on in the patient by the instrumental preparations of his surgeons. From this moment the patient recovered.

It is time to close this communication, already extended to too great a length. It may be worth while however to mention, that up to the time of my writing (20 August 1828) my patient has continued in good health, having long since regained her flesh and strength.

Article VII.—*Observations on the Removal of Foreign Bodies lodged in the Œsophagus. Read before the Philadelphia College of Physicians* 30 *July* 1828. *By* Henry Bond, M.D.

[With a Plate.]

There are few medical practitioners who have not been perplexed with cases of a foreign body lodged in the passage from the mouth to the stomach. And it must be admitted, that the subject has been treated of by surgical writers more vaguely than would seem to us consistent with its importance; whether we regard, on the one hand, the surgeon's perplexities and failures, or, on the other, the patient's distress and danger. Cases often occur where the result is fatal; and perhaps there is no one where the practitioner is oftener unsuccessful in fulfilling a clear indication. This is not, I apprehend, owing to any intrinsic and insurmountable difficulty; and had it received as much attention as some others of not more importance, the practitioner might be charged with mal-practice, who should fail of giving prompt relief to his patient. Some who have written systems of practical surgery have wholly omitted the subject. One reason of this neglect perhaps may be, that in the treatment of these cases reputation is neither acquired nor lost. They are much less attractive to popular admiration than those cases where there is a display of knives, and gashes, and blood. As one proof of this, it may be remarked, that the dealers in terrible accidents and operations seldom deem these cases worthy of a place in their chronicles.

Foreign bodies are usually arrested so high in the fauces or pharynx, that they are visible upon depressing the tongue, and looking into the throat. In such cases relief is usually afforded without difficulty by means of the fingers or a dressing forceps. But when the body has passed into the œsophagus, beyond the reach of the eye and the finger, the difficulty is much increased. The difficulty and danger of the case will also depend very much upon the size, form, and composition of the body. In some instances if relief be not afforded by the prompt removal of it, the patient is suffocated, and perishes immediately. Does this result depend upon the mechanical obstruction to the trachea, or upon the spasmodic constriction of the muscles of the glottis? Mr CHARLES BELL says, (and the opinion was adopted by Dr DORSEY) that, "it is not the mechanical obstruction to the trachea which we have to dread from bodies sticking either in the trachea or œsophagus; it is the spasmodic contraction of the muscles of the glottis." After enumerating all the substances which are liable to lodge in the throat, he says, "*all these* endanger suffocation by exciting the muscles of the glottis." This opinion may be true so far as it applies to foreign bodies in the trachea; but as it regards the pharynx and œsophagus, it is confirmed neither by my observation, nor by the cases which I have found recorded, or which have otherwise come to my knowledge. In nearly or quite all of those cases where immediate suffocation takes place, the body is of sufficient magnitude to produce a mechanical obstruction. The patient, through fright or voraciousness, attempts to swallow a large piece of tough meat, or other substance; the epiglottis falls down; the foreign body descends upon it, and becomes impacted between the horns of the os hyoides and those of the thyroid cartilage, where it is held by the efforts of the muscles of the pharynx to carry it down. Where there is a spasmodic closure of the glottis produced by the irritation of a foreign body, there will be a spasmodic effort of the chest to throw it off by coughing. If coughing could be induced in the case we are contemplating, there is very little doubt but the body would thereby be dislodged. But the mechanical pressure upon the epiglottis prevents the inspiration, without which coughing cannot take place, at least with any considerable impetus.

When suffocation takes place, in consequence of the inflammation and tumefaction caused by the pressure of a foreign body, the result is not attributable to the irritability of the glottis, but to the mechanical obstruction produced by the swelling. Where death occurs suddenly, after the lodgement of a large body at the top of the larynx, the glottis does probably contract spasmodically; but if the body did not act mechanically, in the manner above described, coughing would be induced, and the body dislodged. A barb of rye or wheat, or other small body, may become fixed in or near the glottis, and irritate so much as to lead to a fatal result by coughing or otherwise; but I know of no instances where suffocation has been produced by pins, needles, fish bones, or the like, irritating the glottis to contract spasmodically.

These views seem to me to be legitimate deductions from the anatony and physiology of the parts, and from the history of cases as far as they have come to my knowledge. But I forbear any further discussion of this point, as I began this communication with the intention of making only some practical remarks on the means and methods of treatment. This will necessarily vary, according to the properties of the body, and the part of the canal in which it is arrested. This most commonly takes place in the middle or lower part of the pharynx, but it may occur at any point between the palatine arches and the cardia. If the body be large, it will not descend to the narrower or lower part of the pharynx, but will be fixed at, or near the top of the larynx. It is the lodgement of a large body at this part, which occasions nearly all the sudden deaths which occur from foreign bodies lodged in this canal. Death occurs so suddenly in these cases that medical aid is seldom obtained until the patient is beyond its reach. Were a person at hand at the occurrence of the accident, who understood the nature of the case and the remedy, relief would be easy and certain; for in these imminently dangerous cases, the body will be found, as before stated, locked between the cornua of the os hyoides and thyroid cartilage. From this position it may be removed by the finger or by the common dressing forceps, if the curved forceps are not at hand. If the foreign body be in any other part of the pharynx, there will be much less immediate danger.

In most cases where a foreign body is to be removed from the throat by the fingers or by other means, it would be advisable to put a piece of soft wood or cork between the teeth. This advice is, however, of less importance to be observed, than that of Mr C. **Bell**, who says, " we ought not in an impressive and rapid manner to seize upon the patient; for this puts him into terror and anxiety, that increases the difficulty of breathing. Without much loss of time, it is possible to sit down before him in that composed manner which will give him confidence of relief."

If the body is to be removed by the fingers, let the patient's head be erect, and held in such a manner as to make the chin project as little as possible beyond the pomum Adami. This will enable the operator to carry his finger considerably farther into the pharynx than when held in the natural position, and he can with ease reach that part where the body lodges. If the body descend beyond the reach of the finger when held in this manner, it will be too low to endanger an immediately fatal result, unless it lodge directly opposite to the top of the sternum. By pressing on the posterior part of the thyroid and cricoid cartilages, a large foreign body may produce some difficulty in respiration, but very seldom to a dangerous extent, as those cartilages have so much firmness, that they would be shoved forward or to one side, before they would be so compressed as to stop respiration. The trachea is much more easily compressed, but it is also surrounded by parts so loose and lax, that a foreign body in the œsophagus would displace the trachea, before it would compress it so as to interrupt respiration, except at the top of the sternum, where it might take place to a dangerous extent. This view is supported by a case recorded in the 3d volume of the Dublin Hospital Reports. Something had stuck in the throat of an aged woman while eating, which nearly produced fatal suffocation; and on inquiring where she felt the distress, she pointed to a particular part of the throat which appeared to be directly *opposite to the top of the sternum.* Mr Howship "passed a probang down her throat to the piece of meat forming the obstruction, and pushed this substance *nearly down* into the stomach, when relief was immediately obtained, it being evident that the mechanical pressure of the trachea had been the sole cause of the severe symptoms." I might, with propriety, have referred to this case as confirmative of the views which I advanced when examining Mr Bell's opinion of the cause of suffocation. The efforts of the finger may be aided when the body is large by grasping the trachea and œsophagus with some firmness immediately below the point where the body rests, and at the same time shoving the hand upwards. This grasp, for obvious reasons, should not be long continued. If the body cannot be removed by means of the finger alone, let the tongue be depressed by the fore finger of the left hand on a spoon handle, and the operator will be able to see the body and grasp it with a forceps. If a gula-forceps is not at hand, the common dressing forceps will answer the purpose. Any person with self-possession and an ordinary share of manual dexterity might be very readily taught how to afford relief in such cases, although possessed of no anatomical knowledge or surgical skill.

Besides the larger bodies, to which the preceding remarks have more special reference, other bodies, such as needles, pins, bones, &c. may lodge in the fauces or pharynx without endangering suffocation. These cases are, however, not always free from difficulty and danger, as may be seen by referring to baron LARREY's account of the case of M. Esteve*. The difficulty in such cases arises principally from the smallness of the body allowing it to be concealed in some sinus. This at least was the fact in the case referred to; but there are, I apprehend, few such lurking places about the fauces, which could not be explored by the finger or the eye.

When the foreign body descends into the bottom of the pharynx, or into the œsophagus, although the immediate danger is lessened, the operator's task becomes more difficult, and the difficulty increases in proportion to the depth to which it descends, unless the body be such as may with safety be forced into the stomach by means of a probang. So great, indeed, has the difficulty been considered, that the idea of extracting a body from the œsophagus through the mouth by instruments seems to have been almost hopeless among surgeons. When a body has passed out of sight, or out of the pharynx into the œsophagus, Mr B. BELL says, "No advantage can be derived from the forceps; and we are in such circumstances reduced to the necessity of either allowing it to remain where it is; of pushing it into the stomach; or of extracting it by laying the œsophagus open." "In every case of obstruction of the œsophagus," proceeding from a foreign body being fixed in it, he says, "it is almost the universal practice to endeavour to push it into the stomach."

Mr BELL indicates the cases where he thinks this would be "the most prudent method of getting free from it;" at the same time pointing out the danger of the indiscriminate use of the probang, as well as the mischief which may result from the use of strong vomits. It will be seen by referring to his Surgery, that he never contemplated the removal of a foreign body from the œsophagus by forceps, or any other instrument except the scalpel; and we are directed to make no attempts to remove it unless the symptoms are very urgent. We must wait for the body to decompose, or until the œsophagus ulcerates enough to give it an outlet. If however the symptoms are too urgent to admit of this delay, we must, as he thinks, proceed to œsophagotomy. Mr ALLAN BURNS entertained nearly the same opinion as Mr B. BELL of those cases where a foreign body gets below the pharynx. "But if it stick just at the top of the œsophagus," he says, "it is there too low to be laid hold of by the fingers; and even the curved forceps can hardly be so applied as to extract the foreign body; neither will the probang enable us to force it into the stomach; or granting that it would, we may have reasons for not wishing to place it there." If, in such a case, the foreign body occasions very unpleasant symptoms, he says, "there ought to be no hesitation in performing the operation of œsophagotomy." It was the opinion of Dr DORSEY (which we presume was also that of Dr PHYSICK) that foreign bodies can be extracted through the mouth, only when they are situated high up near the fauces. "If near the stomach, they must," says Dr DORSEY, "be pushed down, and the risk of the consequences incurred."

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* See HALL's translation of LARREY's Memoirs of Military Surgery, Vol. I. p. 142.

Some surgeons, on account of the dread which they attach to the operation of cutting into the œsophagus, which they think should almost never be performed, have proposed several instruments or contrivances for the extraction of foreign bodies from this canal. The success of practice in these cases shows that the patient is very often obliged to trust to *vis medicatrix naturæ*, rather than to these contrivances for his safety. I shall first notice in a brief manner the means which have been proposed for extracting foreign bodies from the œsophagus, and then propose some of my own, which I venture to hope may be considered improvements in the treatment of such cases.

The instruments which have been recommended, or are now in use in such cases, are forceps, hooks, compressed sponge, and loops of thread or horse hair.

It is hardly proper to include the *forceps* among the instruments employed for extracting foreign bodies from the œsophagus, as I have already shown that it is generally concluded to be inapplicable to such cases. The crooked, or gula-forceps (fig. 2) is well adapted for the removal of large foreign bodies from the pharynx; but for extracting bodies from the œsophagus, it is defective in several respects. It is too short; its curve is such that it would be next to impossible to carry it down that canal if it were long enough, being curved in such a manner as to be accommodated to the course of the canal when the head is held in a natural position; the blades come into opposition by broad surfaces, terminating in an acute prominent edge, like two half cylinders; so that when the instrument is passed down the throat we are almost sure to pinch some of the surrounding soft parts, if we attempt to grasp any small body; if we grasp a pin or needle, the teeth or deep transverse grooves in the blades render it necessary either to bring out the body transversely, something like dragging a fish-hook through the fauces, or to run a great risk of losing our hold of it. On referring to a large volume of plates of surgical instruments, published at Vienna, by G. A. Brambilla, I find some crooked forceps considerably longer and heavier than those now in use here and in England, but liable to the same objections, and from their shape evidently not intended to be carried below the pharynx. The crane-bill forceps, recommended by Mr C. Bell, is exempt from none of the foregoing objections. In short, we may say that these instruments cannot be trusted out of sight. The forceps recommended by Mr J. Hunter for extracting foreign bodies from the urethra, has been recommended also for extracting small bodies from the throat. It would be less liable to pinch the contiguous soft parts, but in every other respect it seems to be decidedly inferior to the common dressing forceps*. The *ostagra* of professor

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* In the Medico-Chirurgical Transactions, Vol. XI. page 359, there is an account of a modification of Mr Weiss's bullet forceps, by which it was converted into an instrument for the extraction of small calculi from the bladder. Mr Samuel Cooper, referring to this instrument in the fifth edition of his Surgical Dictionary, says "no instrument seems better calculated for cases in which the body lodged in the œsophagus is not too wide." It does not however appear to me superior in any respect to Desault's modification of Hunter's forceps, either for extracting urinary calculi from the bladder, or foreign bodies from the œsophagus. And, by the way, we cannot forbear to express our surprize, that Sir Astley Cooper should have been ignorant of the practice of Desault so late as 1820, six years after it had been circulated in the United States in an English translation. In size and shape, the forceps above recommended by Mr

VENEL was contrived to be employed in these cases; but it is a complicated awkward instrument, whose place can, in every instance, be better supplied by some other.

Every writer at present, who treats on this subject, recommends the use of a hook. It was formerly the custom to attach a small steel hook to the handle of a probang, or to a piece of whalebone, by means of a piece of watch-spring. But it is of late commonly recommended to be made of a doubled and twisted wire, with a loop at the end bent into a hook. We find a representation of its figure in some surgical works, and nearly the same in each. Fig. 5 is the same as that in Mr S. COOPER's First Lines, and does not differ essentially from that represented in Mr C. BELL's Operative Surgery. There are very few cases to which such a hook would be well adapted. It is ill suited to extract a coin, as such a body would be so liable to slip out of it. It would take no hold of a pin, needle, or fish-bone; for when these bodies are lodged in the œsophagus, they have descended point foremost; the point catches in some ruga or lacuna, and the efforts made to carry them down into

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S. COOPER, has almost an exact resemblance to a large male silver catheter bisected longitudinally about three inches and a half from its point. These two halves or sections are opened by a stilet sliding in the inside; and are closed only by their own elasticity. Its curvature will not allow it to pass much below the bottom of the pharynx; and if it should be forced lower down, the point would press so hard against the anterior part of the œsophagus, that there would be very little chance of seizing the body, and every probability of embracing a large fold of the canal in closing the blades. There is no way to regulate the force applied to the body which it embraces, and it must be small, or that stilet would not open it. The blades form so large an angle with each other when opened, that no body would be held securely except the small calculi, for which it was intended, or other small globular bodies. As an œsophagus forceps, it is therefore in no respect superior to the common curved forceps. It is true, it has no joint; but when a forceps is carried down into the œsophagus, there is so little motion in the joint when the blades are opened or closed, that there is not much probability that the soft parts will be injured there.

In PERRET's *L'Art du Coutélier*, plate 98 is the representation of a forceps designed for the removal of foreign bodies from the œsophagus. It is called *acanthobolon* by PAUL, HILDET, and others. Excepting its curvature, it is made exactly like the common bullet forceps. The instrument is straight until we pass an inch and a half beyond the joint. The rest of the blades are bent so as to correspond nearly with the quadrant of a circle whose diameter is eight inches. Its curve is therefore more favourable to a deep introduction into the œsophagus than any other of which I find any account. Its defects may be perceived from what I have already said of others.

the stomach have often driven the point into the side of the canal, so as to adhere with considerable firmness. The upper or obtuse end of these bodies is generally loose or floating; that is, it is not fastened into the opposite side of the œsophagus. This doubtless is not always the case, but how often both ends are fastened in opposite sides of this canal it is not easy to determine. Under either of these circumstances there is not a great probability that such a hook as that represented by Mr Cooper would dislodge one of these bodies, and none that it would extract them by the mouth. All that Dr Dorsey says upon the use of hooks, is, "that a hook of curved wire is also useful in some cases for extracting foreign matters, especially when they are of considerable size." Upon this opinion, unsustained by experience as far as I know, it may be remarked, that if the body has lodged in consequence of its size, it would be very difficult to pass a hook below it, especially if the hook were large enough to take hold of "a body of considerable size." There is therefore every probability that a hook of the ordinary form would act as a probang. Besides, let a person attempt to apply such a hook to any body of a regular or rounded form, or a smooth surface, such as a nut, a peach-stone, a lump of meat or potatoe, and he will in a moment see how improbable it is, that such a hook would extract such a body. If the body to be removed had any holes or projections upon which the hook could fasten, and would at the same time allow the hook to pass below it, in such a case the hook might be useful; but under this restriction its use must be very limited.

Mr C. Bell says, "Nothing is so good to pull up bodies from the œsophagus and pharynx as a piece of wire twisted and bent into a hook, as I have represented in the plate. The advantage of this simple instrument is, that it can be made of any form, or take any curve, enabling us to adapt it so as to operate upon that side where the pin or bone is supposed to lodge." We admit the superiority of the wire hook over others and for the reason he has assigned; but we are extremely doubtful of the ability of Mr Bell or any one else "*to pull up* from the œsophagus and pharynx" either a pin or stone with the hook he has represented.

For the extraction of a pin from the œsophagus, he has proposed to carry loops of horse hair down to it on the end of a wire through a hollow tube, in the expectation of entangling the head of it. The probability of the success of this contrivance is sufficient to warrant a trial of it, when other more certain means are not at hand. For the extraction of needles and fish bones, he offers no rules nor means which encourage us with any prospect of success. Neither his forceps, nor his hook, nor his loops are applicable to such cases.

Mr S. COOPER's ingenious method of using compressed sponge seems to me more extensively applicable, and to hold out a greater prospect of utility, than Mr BELL's loops of hair. It would, however, much more frequently dislodge small bodies that become fixed, than extract them through the mouth. One objection to it is, that if the first trial were not successful, it would require a considerable time to prepare the sponge to repeat the experiment.

It is easy to conceive of cases where it would be hazardous to resort to vomiting for the removal of foreign bodies; for they may have such sharp points or edges, or raggedness, that we should run a great risk of cutting or lacerating the passage by their violent action. A very small portion, however, of those bodies, which lodge in the œsophagus, have a form which precludes the use of emetics. They promise to be more certainly useful, where the body is of a regular or rounded form and smooth surface, and is arrested in the passage on account of its magnitude. If a small body, having only one rough or sharp point (such as a pin, or one half of a round bone of a bird or chicken), be arrested in the passage, we may always presume that it has descended point foremost. In such cases emetics may be used with impunity; but, if the descending point be so slender as to penetrate with ease the side of the canal to a considerable depth, we shall use them with small prospect of success. Such a piece of bone as that just mentioned would without much doubt be thrown up, as the point would not penetrate far, and it would be of such a size, that both the contents of the stomach passing up, and the inverted action of the canal would have some power over it. When the sharp slender point of such a body as a pin or needle penetrates the side of the œsophagus, the direction of the body is as nearly transverse to that of the canal as its length will admit of; but when an emetic operates, the pin is brought nearly parallel to the axis of the canal, which will occasion the point to stick more firmly, and at the same time neither the contents of the stomach passing up nor the inverted action of the œsophagus can act with any force upon it. Hence pins can seldom be removed by this means, especially if they have been lodged a considerable time, and attempts have been made to carry them into the stomach. I have repeatedly employed vomits in such cases, and never with success; but I once removed part of the clavicle of a chicken immediately by the use of sulphate of zinc. The above remarks will apply to a needle, except for the circumstance that the two ends of it, especially if it be small, may fix in opposite sides of the canal. In such a case, an emetic will cause the body to be more firmly fixed; but if the needle be long and large we need not dread this accident, for it will remain nearly parallel with the course of the canal. An irregular rough fragment of bone would perhaps never be removed by the mere inverted action of the œsophagus, but it might sometimes be forced up by vomiting, if this should be induced soon after swallowing a considerable quantity of solid food. The success attending the use of vomits for the removal of *coins* from the œsophagus is not such, so far as I can learn, as to encourage a resort to them. A case occurred within a few days in this city, where vomits were repeatedly tried without success in the case of a child who had swallowed a half cent. After a coin has been in the œsophagus for some time, the part where it lodges has, from continued distention, lost its contractility; the parts above and below contract, perhaps the more on account of the neighbouring irritation; a sort of pouch or sinus is formed for the foreign body, and, as the natural position of the coin would be vertical, there would be space enough for the contents of the stomach to pass up without dislodging the coin.

Two cases are quoted in the Dict. des Sciences Med. Tom. VII. p. 21, 22, in which persons, at the very point of suffocation from the lodgement of large pieces of tough meat in the pharynx, were relieved by injecting a solution of tartarized antimony into a vein in the arm. In DORSEY's Surgery two cases are noticed, in which Dr PHYSICK removed peach stones from the pharynx by directing the patients to take a solution of tartarized antimony in the mouth and to attempt to swallow it. These and other similar cases show that it would be very unwise to discard emetics from among the means of treating such accidents.

Another means of relief deserves to be noticed, as it has succeeded in cases where other means failed. As professor RICHARD W. HALL, of Baltimore, is the only writer by whom I find it mentioned, I will present it in his own words. It is contained in a note to his translation of LARREY's Memoirs of Military Surgery, Vol. I. p. 143. " When a small bone is lodged in the fauces or œsophagus, it may be decomposed, or rendered so flexible that it will pass into the stomach, by taking frequently of diluted mineral acids. They should be taken through a tube to prevent their action on the teeth. The strength of the acid should be accommodated to the sensibility of the parts over which it must pass. By this means I have succeeded in removing a small chicken bone from the œsophagus, across which it had remained firmly fixed for several hours, although an emetic had been administered, and the curved forceps and probang had been repeatedly used without success. Experiment shows, that even the acetic or acetous acid will decompose the bones of fish, and leave them in a pliant and gelatinous state."

Although forceps have never been carried deep into the œsophagus, a slight attention to the subject is sufficient to satisfy any one, that there is nothing in the structure of this passage to prevent the introduction of an inflexible instrument to any depth. I have already pointed out the defects, as they appear to me, of all the forceps which have been recommended for the extraction of foreign bodies from this passage; and these defects may all be traced to two erroneous ideas, viz. 1, That a forceps cannot be trusted below the pharynx for fear of pinching or lacerating the soft parts; and 2, that the bend of the instrument must be adapted to the course of the passage when the head is held in its natural position. While held thus it would be impossible to carry an inflexible instrument to the bottom of the œsophagus; but by carrying the head back to no dangerous or painful extent, even a straight instrument may be carried down into the stomach, as is evinced in the performances of jugglers, who swallow swords, &c.

I have caused a forceps (fig. 1) to be made, which seems to me to possess some advantages which do not belong to any other.

1. I have given it such a curve, that when the head is held back in a proper position, it may be introduced into the œsophagus to any depth, even into the stomach. Whether in this first attempt I have succeeded in giving it that curvature which upon further trial shall be found best, I cannot assert; but my object was to make it such that it would pass down with facility when the head is carried back, so as to make the axis of the whole passage from the mouth to the stomach approximate as near to a right line as could be done without giving the patient inconvenience. In some trials with it, I found it could be introduced with great ease. An inexperienced hand would be less liable to carry its point into the larynx, than that of the common curved or gula-forceps. Should experience hereafter

indicate any change in its shape, it probably will be to make it less curved.

2. The next advantage which it possesses, is, that the blades may be opened and closed in the œsophagus without pinching or otherwise injuring the contiguous soft parts. This object is fully accomplished by the manner in which the edges are ground or beveled off, and by the sheath of elastic gum which embraces the joint without impeding its motion. The blades of the forceps are so long and can be opened only so short a distance in the œsophagus, that there will be very little motion at the joint, and in almost every case I should therefore consider the sheath superfluous. Should any one consider the instrument unsafe without it, it should be a little longer than the joint, and may be made by sewing with the glover's stick a thin piece of gum elastic, of the proper dimensions; or it may be made more neatly by adopting the method for making a tube, which is recommended in Ure's Chemical Dictionary. (See Art. *Caoutchouc.*)

3. Another advantage is, that by means of the narrow line of rough surface by which the blades come in contact, it will hold such a body as a pin, needle, fish-bone, &c. more securely than the common curved forceps, and extract it with little or no danger of lacerating any part of the passage. At the same time that it holds the body fast, it allows it to vibrate freely, and to assume a position nearly parallel to the bladés. For the extraction of a coin its adaptation seems to be perfect. In short, it appears to be more especially adapted to the removal of those bodies which are arrested on account of their form, and not on account of their volume. When a body, without any sharp points or projections, such as a chesnut, a small apple, or piece of meat, is lodged in the œsophagus, we may presume that the canal, at that point, will be so distended that there will be some difficulty in passing the blades below the body. This probably will seldom be practicable where the body has been forced down by the bold application of the probang. But where the body has been carried only so far down as it could be done by the action of the muscles, it is probable that by opening the blades so as to distend the œsophagus considerably, just before they reach the body, they might be carried below it. By opening the blades in this manner they would necessarily pass down on opposite sides, so as to bring the body between them, and embrace it securely, unless it were vitreous or very hard and polished. But in most cases of

large, regular, or smooth bodies lodging in the œsophagus, if there be any difficulty in extracting with instruments, we may resort to emetics with very strong hopes of success; for it is to such cases that they are more especially adapted.

Although I believe there are few cases where the need of a hook will not be superseded by the forceps here presented to the profession: yet, as I have pointed out the defects of such a hook as we find in the books, I take the liberty to offer some modifications, as hooks may be made extemporaneously when a forceps is not attainable. The hook fig. 3 is intended more especially for the extraction of pins. It may be made of doubled wire, but I should prefer it single, similar to the plate. A single wire is sufficiently firm for all the force it would be prudent to employ; it presents a smoother surface to the soft parts over which it passes than a doubled and twisted wire; it is more easily bent so as to act in different directions; and perhaps there would be some small difference in its favour in the facility with which it would catch the body sought for. If it should be thought that the lower end of the hook, on account of the shortness of the bend, presents too sharp a point to be passed with safety down the œsophagus, cover the point with a drop of sealing wax. This precaution would be needless in hands of ordinary dexterity,—and destitute of it, no one ought to meddle with surgery. The space between the ascending and descending parts of this hook should be accommodated to the size of body to be extracted. In general it should be such as to admit the body of a brass pin with ease, without allowing the head to slip through. The two parts of the hook should be parallel to each other, about half an inch upwards from the bend; otherwise there will be danger, as soon as we have caught hold of a pin, that it will slip out and pass into the stomach, as I have repeatedly experienced; and, instead of extracting, we shall only dislodge it. With this hook there would be a better chance of dislodging, if not of extracting, a needle than with any other; but even with this it would be a doubtful experiment, and time ought not to be wasted where the forceps are procurable. The softer texture and more uneven surface of most other long and slender bodies render it much more probable that they could be extracted by this hook. To prevent the escape of a body from this hook, a very simple addition to it might possibly be found useful in some cases. I do not propose it, however, with much confidence in its utility. Let a wire, similar to that of which the hook is made, be bent at one end into a ring, just large enough to slide upon the handle of the hook; and let the wire to which this ring belongs slide through another ring at the top of the handle of the hook. By dropping this sliding ring upon a body, when caught in the hook, there might, in some cases, be a greater probability of extracting it.

For the extraction of a coin from the œsophagus, the lower part, if not the whole of the hook (fig. 4) should be a doubled wire. The descending and ascending parts of it should be only far enough apart to admit the coin with ease, and the two parts of the wire at the bend should be so far apart that it would not be apt to roll out. The descending and ascending parts of the hook should be parallel to each other, so far as to sustain the coin in a vertical direction; but the point should stand off so far from the handle as to make it easy to catch it. A hook made in this manner would have some advantage over that which was presented to the profession two or three years ago by Dr NATHAN SMITH of New Haven. A coin would be less liable to escape from it, on account of the much broader resting place for it at the bottom; the opening of it is such that it would more readily take hold of the body; the handle may be readily bent so as to act in any direction; and it may be made extemporaneously out of a piece of common wire.

Although a probang may in some instances be used with safety, I apprehend that surgery would sustain no essential loss if it were entirely discarded from its apparatus; for in every case where it would be applicable, the patient would be relieved with more ease and safety by the forceps, or by vomits. In those cases to which a a hook is applicable, the probang should seldom or never be employed. Whether, therefore, it be advisable to retain among our apparatus an instrument which is never necessary, and which is so liable to be used in improper cases, it is not for me to decide; and whether any thing contained in this communication be entitled to the distinction of an improvement in surgery, I leave to be determined by this very competent tribunal, and by a profession whose test is experiment.

Explanation of the Plate.

Fig. 1 is drawn from a forceps twelve and a half inches in length, which will reach two or three inches below the top of the sternum. For reaching the bottom of the œsophagus in the adult, the instrument should be two to four inches longer. The curve corresponds nearly to a circle, whose diameter is twenty-four inches. It does not, however, exactly correspond to a circle, for the curvature lessens in passing from the handle to the point. The joint should be distant from the point not more than three-fifths of the length of the instrument, and be made long, in order that the blades may with more certainty come exactly in opposition. The edges of each blade are ground away with such a bevel that the blades present to each other, where they come in contact, an obtuse angle of about 130 degrees. This angle on each blade is made rough by cutting it into a row of teeth, either by a chisel or a file. The point of each blade is ground off like the edges, so that the row of teeth is not continued quite to the point. This figure is half size.

Fig. 2 is an outline of the common curved or gula-forceps, presented here to illustrate its defects by contrasting it with my own. The curvature commences two inches beyond the joint; and from that point it forms the sextant of a circle, whose diameter is six inches. This might with propriety be named the *fauces forceps*. It is half size. See page 284.

Fig. 3 is the hook which I propose for the extraction of pins, &c. It is of the full size, and sufficiently explained at page 291.

Fig. 4 is the hook proposed for the extraction of coins from the œsophagus, and is of the full size. See page 292.

Fig. 5 is copied from the hook represented in COOPER's First Lines. See page 285 of this communication.

Fig. 6 represents a transverse section of the blades of the forceps fig. 1.

ARTICLE VIII.—*Observations on Cancer of the Breast. By* JOSEPH PARRISH, M.D., *One of the Surgeons to the Pennsylvania Hospital.*

In a former paper I gave my views relative to certain morbid conditions of the breast which might be mistaken for scirrhus:—in the present my object is to make a few observations upon the genuine disease, as it appears in that organ.

It usually commences with a small, distinct tumour; which in some persons runs its course with great rapidity; in others, remains indolent for a long time, for months or years, sometimes for many years. When it becomes active, the first symptom is generally a lancinating pain shooting through it. The patient also sometimes complains of an exceedingly distressing, burning pain. The tumour increases in size, and, as it augments, embraces the neighbouring parts, which it identifies with its own peculiar structure. When it reaches the surface, its character becomes more obvious. There is commonly a wrinkled or puckered state of the skin, which I consider as one of the strongest evidences of the cancerous nature of the affection. The skin also changes colour, becoming of a dark red; and ulceration at length takes place. The ulceration, so far as I have observed, is superficial. There is no abscess, nor genuine pus, the discharge being of an ichorous nature, and exceedingly offensive. The odour of the secretion from cancerous ulcers is peculiar, as much so as the mercurial fœtor, or that of small pox. An experienced surgeon will often be able to recognize the complaint upon entering the door of the chamber. Hectic fever now comes on, attended with great nervous irritation and extreme suffering. Sometimes the complaint extends backward, involving the fascia of the pectoral muscle, and the muscle itself; and the tumour no longer remains moveable as when the mamma alone is affected, but becomes fixed by union with the parts beneath it. The absorbents also take on disease. The glands in the axilla enlarge; the disordered action extends to the glands of the neck, and travels into the chest; and, in some cases, almost the whole system is affected before the patient is relieved by death.

I have stated that the complaint occasionally passes into the lungs. Several instances of this kind have come under my notice. They were accompanied with dyspnœa, which was at first paroxysmal as in asthma, afterwards became more constant, and in the end was unremitted and almost insupportable. When a very young practitioner I witnessed the case of a female upon whom a very respectable surgeon of this city operated. The fascia of the pectoral muscle was affected, and the case was pronounced very unpromising. I attended after the operation, and regularly dressed the wound. Good pus was secreted, the cavity gradually filled up, the granulations were healthy, and the prospect was for a time flattering. But by degrees the granulations lost their healthy aspect; dyspnœa came on, at first paroxysmal, afterwards constant; and, for many weeks before her death, the patient, unable to lie in bed, was compelled to sit supported in a chair. In another similar case I had an opportunity of making a post mortem examination. The lungs were found full of tubercles, which, when opened, presented the striated structure peculiar to cancerous tumours.

The wrinkled state of the skin to which I have alluded as strongly characteristic of scirrhus of the mamma is not always present. I have seen several cases in which the gland, instead of being contracted, was considerably enlarged, the skin being smooth, tense, and glossy, as if stretched by the expansion of the parts beneath it. In these cases the surface of the breast presented a peculiar stellated appearance, being spangled with little red spots, arising from a disordered state of the cutaneous vessels. In one instance of this kind, the patient experienced much less pain than might have been expected. Sometimes instead of this spangled appearance there is a uniform dark lurid colour of the breast, the gland being much enlarged and of a stony hardness.

I have also witnessed another condition of the skin attendant upon scirrhus of the breast, to which my attention was first called by Dr Physick. Upon running the finger over the surface, you perceive that it is granulated; and a sensation is imparted as if the skin were filled with small shot. I believe that Dr Physick considers this as the most dangerous form of the disease. He told me that he had never known an operation to prove ultimately successful; and my experience fully coincides with his. I once operated upon such a case in the Pennsylvania Hospital. The disease was confined altogether to the breast, and as far as could be ascertained by the sight or the touch was entirely removed. The wound healed kindly, and for a short time the patient appeared comfortable: but in a few months the complaint returned, and soon terminated fatally.

In the above account of the symptoms and progress of cancer in the breast, I do not pretend to great minuteness of description. I wish only to call attention to the more prominent and characteristic traits of the disease, by which it may be distinguished from affections which, though sometimes mistaken for it, are in fact much less serious in their nature, and call for a different treatment and prognosis.

I have never met with cancer of the breast at a very early age, and seldom among very old people. In the majority of cases it occurs after the thirtieth year and about the period of the cessation of the catamenia. I have seen it in males as well as females, though very rarely in the former. A case of the kind fell under my notice in the Pennsylvania Hospital, upon which my friend Dr JOHN RHEA BARTON operated. The disease, which had reached the axilla, returned after the operation, and proved fatal.

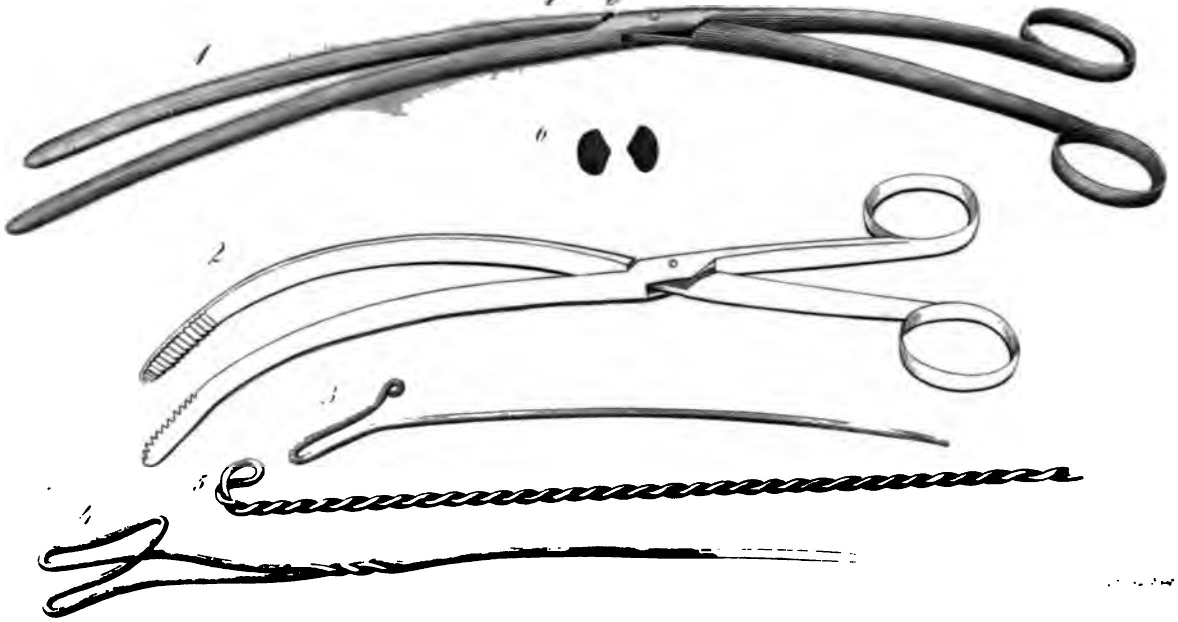
Observation has led me to the belief that cancer is a constitutional affection. We often hear it referred to violence done to the part by blows, falls, or other accident; and it is possible that it may be occasionally developed by such means, where a predisposition to it exists in the system: but I doubt whether genuine cancer ever originates purely from external injuries.

As regards the treatment, I have no confidence whatever in the efficacy of any measures, other than a total extirpation of the diseased part. Even the knife can by no means be relied on as an effectual remedy. There can be no doubt that the progress of a scirrhous tumour may be delayed, occasionally even arrested for a considerable length of time, by depletion, low living, and the avoidance of all exciting causes. But when parts have taken on the genuine cancerous action, and have assumed the peculiar cancerous structure, I am convinced that they cannot be restored to a healthy state by any means which are at present at our command. At least it has never fallen to my lot to experience in my own practice, or to witness in that of others, a result so desirable. Cases of cures are occasionally reported in the journals and other medical works; and I am not disposed to call in question the truth or accuracy of the statements, so far as facts are concerned: but considering the comparative rarity of such supposed cures, it is hardly going beyond the bounds of charity to suppose that the judgment of the practitioner may have been mistaken, and affections recorded as cancerous which have partaken of this character only in their alarming appearance, or in the obstinacy with which they had previously resisted all curative measures. Indeed the very account of symptoms which accompanies these statements affords, in some instances, sufficient evidence that such is really the case.

The strong disposition in the constitution of patients affected with cancer to a return of the local disease, even after every vestige of it has been completely removed, renders it a question how far the use of the knife is desirable. When the axilla or the pectoral muscle is involved, the chance of an ultimate cure is, I believe, very slender indeed. The operation may be carefully and skilfully performed, the wound may heal, and the patient may for some time enjoy apparently perfect health: but in the great majority of cases the disease will sooner or later reappear. I have operated in about twelve cases of this kind; in some have completely dissected out the glands of the axilla, and have even gone round the clavicle, removing as far as was discoverable every suspicious part; yet with one or two exceptions the patients have ultimately fallen victims to the irresistible encroachments of the malady. In one instance, in which, however, the tumour was soft and approached to the nature of fungus hæmatodes, a permanent cure was effected. The patient was the wife of a poor sailor. The axillary glands were extirpated together with the breast, the wound healed without difficulty, and though seven years have elapsed she has remained free from the disease. In another case, upon which I operated about two years since in the Pennsylvania Hospital, there has hitherto been no sign of a return. With these two exceptions, in one of which the nature of the complaint was doubtful, in the other a sufficient period has not yet elapsed to test the success of the operation, I have met with no case of cancer of the breast extending into the axilla which has not terminated in death. It generally makes its reappearance within a year after the operation, sometimes, however, not till after a period of two years. The cicatrix is often the part first affected. It becomes painful, and on examination is found red, elevated, and hardened. There is now, I conceive, no chance of escape. I have repeatedly seen the cicatrix removed by a second operation; but never with final success. The result of my experience in these axillary cases, as well as in those in which the pectoral muscle or its fascia is affected is a conclusion never to advise the employment of the knife, as affording a reasonable chance of permanent advantage. When a patient is brought to me under these circumstances, I feel myself bound to inform her candidly of her situation. I lay fairly before her the very slender prospect of an ultimate cure; and state my opinion, that the most she can reasonably expect from an operation is the delay of one or two years in the progress of her disorder. If she then decide to submit to the knife, I do not feel myself justified in withholding it. I never, however, recommend it.

Even when the gland alone is affected we can by no means be certain that an operation will secure the patient. It is undoubtedly true that females affected with cancerous mamma have been entirely and permanently relieved by the removal of the diseased part. Instances of the kind I have witnessed in my own practice. But there is reason to believe that not a little of the credit which the

Bond's Œsophagus Forceps



axilla was free from disease, and the tumour appeared to have no attachment to the pectoral muscle, I was not without hopes that its removal might afford permanent relief. Upon dissecting out this enormous cyst, contrary to my expectation, I found it adhering by a small point to the fascia of the pectoral muscle. The whole, however, was removed; the wound nearly if not quite healed; every symptom of hectic fever vanished; and the general health seemed quite restored. The disease, however, was afterwards renewed in the breast—and, as in every other case that I have observed, in which the pectoral muscle or its fascia was involved, ceased its ravages only with the death of the patient.

On the whole with regard to cancer I am willing to confess, after considerable observation and experience, that I am much discouraged as to the final success of the operation. I repeat that in cases where the disease has penetrated into the axilla, or has fixed upon the parts beneath the breast, I never resort to the knife unless at the particular request of the patient, with a full knowledge on her part of my own conviction as to the probable inefficiency of the remedy. When the breast alone is affected, I advise the operation: but even here I consider it as affording but a doubtful prospect of escape; and however much I may be grieved, am never disappointed at finding my patient again suffering with the disease and finally sinking under it.

Notwithstanding, however, this discouraging view, when I contemplate the goodness of a bountiful providence, and reflect upon the numerous instances in which remedies have been graciously provided for the ills with which we are afflicted, I cannot but entertain the hope and even the conviction that some mode of cure will yet be discovered for this horrible malady: and the man who may become the instrument of this merciful dispensation of divine favour will obtain and deserve an honour among men not less imperishable than that which encircles the memory of the illustrious JENNER.

ARTICLE IX.—*On the Medical Properties of Iodine.* By JOHN BELL, M.D.

Iodine, first used as a medicine for the cure of goitre, has within these few years past been employed in various other maladies, such as amenorrhœa, gonorrhœa, and scrofula. Like every other article of the materia medica, it has been subjected more to the hazardous trials of empiricism than to the guidance of careful observation and methodical induction: and such must ever continue to be the case so long as physicians in search of its specific effects overlook the necessary modifications which it undergoes according to the condition of the tissue or organ to which it is applied, or on which it more immediately and permanently operates. Dissimilar effects consequent on its administration are continually attributed to the intrinsic properties of a drug in place of the difference of circumstances, that is of the animal economy or particular portions of it, pending the time of its administration. Thus a very brief retrospection of the history of medicine makes us acquainted with the immense amount of suffering and waste of lives caused by the empirical use of mercury and bark. Syphilis in certain stages was arrested by the introduction of mercury into the body of the patient; and it was straightway inferred that the alleged poison of the disease had met with its antidote in the medicine. Did a person, however, become worse under the mercurial treatment, the syphilis was declared to be very obstinate and to require still more mercury. Ulcerated throat, cutaneous disease, pains of the joints, nodes, all were referred to the malady;—and although the truth was finally revealed, and it was discovered that all these disorders could be produced by the various preparations of the metal, yet the partial abandonment of one alleged antidote or specific was only the signal for a search after others, and the mineral acids and the preparations of gold were set forth as the means of accomplishing what mercury had failed to perform. The like pernicious maxims were held in regard to the use of this latter in fevers; and with still more fatal results, because the sphere of mischievous operation was so much more extensive. Nothing benefited by these lessons, the majority of our physicians continue still to indulge in a similar error in the employment of the Peruvian bark. Its efficacy in intermittent fever of a certain grade was regarded as a pledge of its virtues in all the varieties not only of this fever but in all periodical diseases. Failures and disasters followed of course the practice of this creed; but the evil was not traced to its source. In place of inquiring whether the human body, suffering under periodical fever, was in that state in which no impressions effected by tonic or astringent articles could be tolerated at all with any safety or advantage to the patient, the medical adviser took it for granted that intermission or periodicity was an unit, and the only questions to be asked were the dose of the bark and the time for its administration. Some wholesale reasoners were indeed disinclined to allow of any restrictions; and whether there was apyrexia, or chill, or fever, the bark was to be poured in. When, finally, some glimmerings of light broke in upon these benighted philosophers, and they were compelled to acknowledge that bark was not a specific for intermittent fever,—what course did they pursue? Inquire into the particular state of the stomach of the patient, compare it with that in which the medicine had been serviceable, and endeavour to bring by other measures the former into the same favourable state as the latter? By no means: they began ransacking first the domain of Flora and then the interior of the earth in order to discover other tonics to relieve the debility induced by fever. Arsenic followed bark, to be in its turn succeeded by iron and copper and zinc and mercury; and thus for the sake of preventing the return of a chill the poor patient is to be slowly poisoned, and left a prey to the wasting effects of chronic gastritis, engorged liver, hypochondriasis and all the horrors of dyspepsia. One would have supposed, by the common rules of logic, that if the hope of benefit to a patient be predicated of the known effects of two co-operative causes or agencies, the disappointment must be sought for in that one of these which is most mobile and exposed to continual fluctuations. The medicine being presumed of uniform intrinsic properties, the inquiry into the failure must then of course be sought for in the other cause, viz. the human body in general or the organ in particular to which it was applied, and the mutations in the state of which are productive of the most diversified results even from the contact of the same substance.

Iodine had been administered in full doses by inunction to a large number of persons for a length of time by Dr Coindet and other physicians of Geneva for the cure of goitre before it was prescribed internally in one of the Italian hospitals for amenorrhœa. The first reports of its effects in this last mentioned disease were of a very encouraging character—menses restored—colour and strength and appetite regained. Not long after this, we received accounts of the injurious and even fatal operation of the medicine; and its use was formally prohibited by a government ever meddling and intrusive, occasionally intent on checking evil, more generally jealous of all improvement. The transition on the part of the medical faculty from exalted eulogy to indiscriminate reprobation, though in character with much of the history of the materia medica, was at direct variance with the principles of physiology. An agent tolerated and even well received by the animal economy at one time cannot by any show of reasoning be proved deleterious to it at all times, and yet this would be the singular syllogism of the prescribers of iodine to whom I now have especial reference.

The first case of *amenorrhœa* in which I employed the medicine was of a female aged thirty years, who had been married and had been pregnant at different times but never had a child at the full term. For three years prior to my visiting her she had suffered from prolapsus uteri and had only menstruated three times during this period. Encouraged by the relief afforded to a neighbour by the introduction of a pessary, she allowed an examination per vaginam in her own case. The pessary was used by her with the effect of affording speedy cure and comfort, and ability to move about and walk with freedom in place of spending, as she had been compelled to do, much of her time in a recumbent posture. The first menstrual discharge was effected three months after the introduction of the pessary, and followed the use of the volatile tincture of guaiacum and balsam copaiba—in the proportion of two parts of the former to one of the latter; dose, a tea spoonful at first three times and afterwards twice a day. This mixture had been taken during the preceding month, and in the last instance for four days before the coming on of the menses. Two months after this she had a menstrual discharge,—having made use for five days before of the mixture as above and an injection per vaginam of aqua ammoniæ in milk three times a day, and pediluvium at bed time. I ought not however to omit mentioning an additional means of exciting the uterus, which was also had recourse to once before the first discharge: it was the introduction of the end of a large gum elastic catheter into the os tincæ, which in this case was wide and the sides soft, full and flaccid, and giving it for some minutes a rotatory movement.

On the following month pills of myrrh, aloes and rhubarb were prescribed, alternating with the volatile tincture of guaiacum; and the injection per vaginam of aquà ammoniæ xv to xx drops in milk, three times a day for five days, with the pediluvium at night was had recourse to: but all without effect.

Two months after this the tincture of the iodine was prescribed.

The patient's temperament is lymphatic—flesh soft—cellular tissue puffed—complexion pale. She is subject to sick headach—tongue usually moist, at times somewhat loaded—pulse little affected. This state, which seemed to authorize the stimulating emmenagogues already given, would, it was thought, justify the use of the iodine. A saturated tincture of two scruples to an ounce of alcohol was the form prescribed*. Of this twenty drops were given twice a day, and the dose gradually increased to forty. After twelve days use it was discontinued, without any other effect than pain in the back and head, and thirst after the first four days. The use of the tincture was resumed in a fortnight in doses of forty drops twice and after a while thrice a day, and continued for fifteen days; during the latter part of which the ammoniacal injection and introduction of the catheter were practised, but all without exciting any menstrual effort. Aloes, rhubarb, and sulphate of iron, in pills, were directed for the next month, and followed by ergot in scruple doses twice a day, and the ammoniacal injection. The ergot, on account of its causing nausea and vomiting, was discontinued. The tincture of the iodine was, after a few days, directed in its place, in a dose of twenty-five drops, to be gradually increased to fifty, three times a day; and the liquor of ammonia twenty drops, four times a day, mixed with milk, injected per vaginam. The face at this time and the extremities were puffed and swelled. On the second day of this last treatment the patient was seized with a severe attack of spasm of the stomach, followed by pains in the back, and on the fourth the menses appeared, and continued for two or three days to flow very freely.

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* Mr Durand, one of our most intelligent apothecaries, states that an ounce of alcohol, of the common strength, will not take up more than two scruples of the iodine. To this tincture I usually direct the addition of a drachm of spirits of lavender compound, which may also be advantageously combined with the solution of the hydriodate of potass. The proportions of which I generally make use are one drachm of the salt to an ounce of water. Where there is any doubt respecting the ability of the stomach to tolerate the medicine, it is safer to begin with either of these preparations in doses of ten drops twice a day.

Three weeks from this time the same measures, by the use as above of the tincture of iodine and the injection, were had recourse to, but without effect. On the following month the same means were equally unproductive of any perceptible results. The use of the sulphate of iron and rhubarb, in pills, for two weeks in the next month, followed by the volatile tincture of guaiacum in doses of a tea spoonful twice a day for two days, produced a menstrual discharge for twenty-four hours. No such effect, however, occurred from the use of the guaiacum tincture given three weeks afterwards, although the pills of sulphate of iron and rhubarb had been taken as before.

No emmenagogue stimulus was given for a period of two months. The first prescribed was a solution of a salt of the iodine, the hydriodate of potass, ʒi. to ℥i. of water. Dose, thirty drops, twice a day, gradually increased to forty, for ten days. The ammoniacal injection was resorted to also, but no uterine secretion was effected. Six days use of the ergot, and canella alba, aa. ℈i. b. d. three months from this time was followed by a menstrual discharge for a day and a night. The like effect succeeded the use of the turpentine during the last spring. The subsequent exhibition of this latter medicine even in larger doses, and for a longer period than that recommended as so successful by some of the late European continental physicians, has not in this case been attended with any beneficial results.

The emmenagogue powers of the iodine have not, from the preceding account, any stronger claims on our confidence than the tincture of guaiacum, the ergot, or the turpentine. As it is only by means of direct comparison that we can duly appreciate the value of a medicine, we have to regret that many of the boasted emmenagogues have not been more fully tried in this way before they were set forth by physicians in terms of such extravagant and idle eulogy. My patient menstruated six times during two years;—a result certainly which affords me no cause of triumph, but which will, I believe, be found as successful as the average relief obtained in such cases. The continued use of the pessary enables her to avoid a train of painful feelings, and to enjoy comparatively good health. Her sedentary habits forbid me perhaps to hope for more.

In the case of a female with chronic gastro-enteritis, and a continued morbid state of the heart—seemingly organic disease of the part, and acute headach, of a year's duration, copious and frequent venesection having been frequently used, without however removing the hardness and frequency of the pulse, the tincture of iodine, as above, was given in doses of twenty drops, twice a day, as an emmenagogue. The medicine was continued for eight days, the dose being gradually increased to thirty drops, but without any beneficial result. A return of irritation of the stomach and some pain in the bowels was the signal for discontinuing the tincture. The associated use of the injection per vaginam of the aqua ammoniæ in milk was of no avail.

In a case of amenorrhœa of three months duration, the tincture of the iodine restored in a short time the menstrual discharge, and freed the patient from fluor albus to which she had been much subjected. Since then for a period of two years she has been quite regular, and free from the pains under which she formerly suffered at the menstrual epoch.

Another female similarly circumstanced used a solution of the hydriodate of potass with equal benefit.

A young woman who had ceased to menstruate for four or five months had the secretion restored after a few days use of the solution as above.

The medicine was given to two young females in the Philadelphia Alms House, in both of whom it brought back so soon the irritable and pained state of the stomach of which they had so often before complained in connexion with their amenorrhœa of long standing as to require of me to desist from prescribing it any longer to them. These patients were doubtless similar to the Italian ones, on whom the iodine was said to have produced such fatal effects as to lead to the entire disuse of the medicine:—they were prone to acute inflammatory attacks and pain of the head, chest and stomach, with great morbid activity of the heart and arteries. My justification for prescribing the iodine to them was the free and frequent sanguineous depletion to which they had been before subjected, and the then comparatively good state of their stomach.

In *leucorrhœa* I have directed the tincture of the iodine in some cases and the solution of the hydriodate of potass in others with speedy and permanently beneficial effects.

In *gonorrhœa* the medicine has at times a very happy operation—soon arresting the discharge. It was the only thing by which I was enabled to cure a most protracted and virulent case of this disease in a young female, accompanied with ulceration of the throat and skin. She had while taking the iodine two returns of very copious menstrual discharge.

Another female advanced in life who received the gonorrhœal infection from a drunken husband, and who had in consequence copious discharge with soreness of the labiæ, was cured in ten days by the solution of the hydriodate of potass ʒi. to ℥i. of water with a little spt. lav. comp. She was relieved at the same time from a distressing pain in the back under which she had long suffered and from an herpetic eruption on her elbows of thirty years duration.

A man who had gonorrhœa, phymosis, ulcers of the prepuce and swelled testicle, was cured by the use of the tincture of iodine in doses of twenty gradually increased to forty drops twice a day; and the application of lunar caustic to the sores. The hardness and swelling of the testicle had entirely disappeared. These salutary effects were brought about in a period of three weeks. After a lapse of twenty days however, this person presented himself to me with a return of his urethral discharge. A solution of the hydriodate of potass as above was prescribed in doses of thirty gradually increased to forty drops twice a day, in place of the tincture, the taste of which was to him so nauseating: an injection of sulphate of copper and opium in water was also directed. This treatment was persevered in for ten days without success. The balsam of copaiba with the cubebs and spirits of nitre in a gum mixture were then prescribed; and in forty-eight hours the running was completely stopped. Warty excrescences on the prepuce were of much more troublesome management, and demanded longer time for their removal than the urethral discharge.

The iodine in some cases of gonorrhœa would seem to merit as favourable a mention as the most approved remedies for this disease; to which in others again it is of manifestly inferior power. It may be received as one of the varieties to which practitioners must often find it necessary to have recourse in this disease, especially if it have been at all protracted before they are consulted on the subject.

In ulcers of the mouth and throat, whether consecutive on syphilis, or proceeding from other causes, I have derived the very best effects from the iodine. Its operation on the mucous surface of the urethra and vagina induced me to make trial of it in ulcers of other parts of the mucous system; nor have I been disappointed in my anticipations. It has been especially serviceable in females of scrophulous habits, in whom troublesome aphthæ and ulcers of the mouth and velum palati appeared contemporaneously with leucorrhœa, and for which mercury is not unfrequently prescribed with very hurtful effects.

I have found the iodine in tincture, or solution of the salt, display very satisfactory curative powers in *scrofulous* ulcers of the neck; and I have frequently prescribed it in cases of *venereal bubo* with apparently the effect of discussing it, or if too far advanced, and that it was opened, of accelerating granulation and cicatrization.

In a case of *enlarged* and *indurated testicle* of long standing, the patient in obedience to my directions made use of the ointment of hydriodate of potass—composed of a scruple of the salt and an ounce of lard—externally, and took internally first the tincture then the solution as above for a month, without any notable change being effected. Venesection from the arm and leeching the scrotum, and fomentations had been resorted to before beginning with the iodine.

I have at this time a patient who is using the medicine both internally and externally for a disease of the hip joint accompanied with a tense tumefaction of the outer portion of the thigh; and pain of the knee, especially at night. Most of the remedies recommended in such cases have been employed in this except moxa and the seton. Cupping very frequently over the hip, and a discharge kept up for a long time from the vicinity of the joint by tartar emetic ointment, and blistering have only afforded partial relief. After a fortnight's use of the iodine the swelling immediately over the joint had nearly disappeared, and allowed me for the first time in many months to feel it quite distinctly. For fear any ignorant philanthropist on seeing these pages should regret that my poor patient had not been recommended to use the popular panacea, I will take this opportunity of saying that he had tried some bottles of it prior to his coming under my care without any benefit whatever.

The apparent impunity with which the animal economy can tolerate large doses of the iodine must not be received as evidence either of its inertness or of the uniform safety of its use. Whatever agent operates so decidedly on the nutritive organs by causing a diminution and removal of morbid growths, as in goitre and scrofulous tumours, or in excessive obesity, must, we may be assured, have a strong and at times paramount power of inflicting great injury. Many mineral and vegetable poisons can, we know, be often tolerated to an extent in some cases which would prove speedily fatal in others. Iodine in small doses will, like many of these articles, exert a tonic power and create appetite, precisely in virtue of its direct effects on organic nutrition, and tendency to make absorption or waste predominate over secretion or supply. This state of the body is almost always accompanied by a craving for food and an energetic action of the digestive passages, as we find in marasmus, except where there is phlogosis of the gastric surface. We shall often therefore be led into very serious errors if we mistake a craving appetite, whether occurring in chronic disease or as a consequence of our remedies, for evidence of the wants of healthful nutrition, or a sign of returning health and strength.

When the tongue is of a bright red at its end and borders, or is very dry and furred, and the stomach hot and intolerant of pressure, or if there be pain or disorder of the bowels, the iodine ought not to be given. In an opposite state of the intestinal canal, with rather a sluggish circulation and lymphatic temperament, this medicine will be found to display effects of unequivocal power and benefit.

Analytical Reviews.

ARTICLE X.—*Traité sur les Gastralgies et les Entéralgies, ou Maladies Nerveuses de l'Estomac et des Intestins. Par* J. P. T. BARRAS, *Docteur en Médecine de la Faculté de Paris, &c. &c. Seconde Edition, revue, corrigée, et augmentée. Paris. Oct.* 1827. *Pp.* 386.

> **Quandò talis amentia et abusus cessaturus est? et quandò aurea mediocritas et sobrietas in territorio medicorum triumphatura?—*Schmidtmann.***

HAVING in several of the previous numbers of this Journal attempted to introduce our readers to an acquaintance with the prominent doctrines of the physiological school of medicine; we conceive it to be equally our duty, as Editors, to lay before them also, as opportunities offer, some of the objections to which these doctrines are supposed to be liable, and the different explanations given by their opponents of the same morbid phenomena. To this intent, we have made choice, on the present occasion, of the work of Dr BARRAS, the title of which is above recited.

Every one in the least degree acquainted with the writings of BROUSSAIS, is aware of the important rank he assigns to lesions of the mucous membrane of the stomach and bowels, but especially of the former, in the production of many of those groups of symptoms, which had been erected by the nosologists into separate and distinct diseases. To demonstrate the incorrectness of this pathology, in reference to various morbid phenomena, and to trace these to their true cause, is the avowed object of the publication before us.

The author, it appears, had laboured for a number of years under a gastric affection; which, he informs us, was invariably aggravated when treated according to the principles of the physiological doctrine, but had its symptoms greatly ameliorated, and was finally cured, as soon as such treatment was abandoned, and one of an opposite character adopted.

These circumstances very naturally led him to a more particular investigation of the several morbid conditions of the digestive organs, the result of which has enabled him, he conceives, to demonstrate that many of those affections referred, by BROUSSAIS and his disciples, invariably to an inflammation, for the most part chronic, of the gastro-intestinal mucous membrane, are in fact pure nervous diseases, that is to say, "lesions of sensibility, without inflammation, or alterations of structure;"—diseases which are always kept up and increased by the remedies adapted to a case of gastro-enteritis.

"We are far, however, from asserting," remarks Dr BARRAS, "that in these cases the nervous tissue is in its normal condition; the disorder of its properties and of its functions announces, on the contrary, that it has experienced a morbid change; but this change is not an organic alteration, nor an inflammation, for our senses cannot detect it." Page 13, *note*.

Dr B. has designated the diseases here referred to by the terms Gastralgia and Enteralgia; uniting under these two general denominations all the gastro-intestinal neuroses; adding the word hypochondriac to indicate those cases which, he observes, are of frequent occurrence, where "a nervous affection of the encephalon is added to that of the primæ viæ."

The nervous affections of the stomach and bowels being supposed seldom or ever to exist separately, the term gastralgia, gastric neurosis, or nervous disease of the stomach, is alone employed in the course of the present work;—the existence of the intestinal disease being however always understood.

It would not accord with our limits to enter into a examination of the long, but very imperfect detail which Dr B. has presented to us of the commencement, progress and termination of his own case. From this detail, however, it is very evident that the disease under which he laboured, whatever we may suppose to have been its real character, was at no period, during its whole course, submitted to any regular or consistent plan of treatment, and that its symptoms and progress by no means warrant the positive conclusions drawn from them by the author. The same observations are, in a measure, applicable to nearly the whole of the thirty-three additional cases related in the work under review. Nevertheless, it is almost solely upon the phenomena of these cases that the pathology of the author in relation to the gastralgia is founded:—speaking of them in his first chapter, he remarks:—

"These facts prove that the physiological physicians are in error in regard to the nature of gastric affections, and that upon this subject, as upon many others, far from having aided the progress of medicine, they have done it a serious injury, in turning it aside from the certain road of observation, to bring it back to the dangerous field of speculation (*systêmes*). In a word, I believe I shall be able to demonstrate by conclusive facts, and plausible arguments, 1. That gastro-enteralgia differs essentially from chronic gastro-enteritis. 2. That the theory in which these two diseases are regarded as identical and requiring the same method of treatment, leads to the commission of very serious errors in practice. And 3. That consequently such a theory is a dangerous weapon in the hands of inexperienced practitioners, or of those seduced by the innovations of the new school."

The subject thus stated is confessedly one of the first importance. If the author has been able to demonstrate that certain symptoms referable to the stomach, are entirely unconnected with inflammation, in any grade, of that organ :—if he has succeeded in drawing a distinct line of demarcation between those gastric affections which demand for their removal an antiphlogistic treatment, and those, on the contrary, in which such a course is decidedly prejudicial, he has conferred an invaluable benefit upon medical science, and will be the means, certainly, of preserving the health and lives of innumerable individuals.

It is proper to remark that Dr B. admits the nervous, like the other tissues, to be susceptible of a state of actual inflammation, and of experiencing organic degenerations.

"We cannot deny," he remarks, "to encephalitis, to mielitis, and to neuritis the right of being ranked among the inflammations: but those diseases which consist only in an irritation, atony or mobility of the nervous system, without appreciable lesion of the parts in which they are seated; that is to say, the nervous affections, which the ancients regarded as a morbid condition without matter, *intemperiem sine materiâ*, and which the moderns, before the period of the physiological doctrine, recognized under the appellation of neuroses; cannot be placed in the same class with the former."

He is willing to confess, also, that most of the symptoms attendant upon nervous diseases are frequently symptomatic of, or in other words, result from, inflammation.

"We are now aware," says he, "that the greater part of those symptoms which characterize the diseases termed nervous, may depend upon an acute or chronic phlegmasia, upon an organic lesion, or upon a foreign body in the interior of our organs: we know, for example, that convulsions and delirium result, frequently, from the presence of worms in the intestines, from a scirrhus of the brain, or ossification of its meninges; from an arachnitis, an encephalitis, or in very irritable constitutions, from any other inflammation. We therefore infer that the essential neuroses are of less frequent occurrence than was formerly believed, and that the diseases regarded as such, for the last forty years, are in fact genuine chronic (*latentes*) inflammations; morbid affections then but little understood, and for having described which, particularly as they occur in the mucous membrane of the digestive canal, with much greater accuracy than they had been before the appearance of his *Histoire des Phlegmasies Chroniques*, M. BROUSSAIS, preceded in this, however, by M. PUJOL, has deserved highly of the profession."

But because many nervous phenomena depend frequently upon a phlegmasia, it does not follow, Dr B. conceives, as a necessary consequence, that the neuroses are to be considered invariably as inflammations; on the contrary, he is persuaded that idiopathic neuroses, that is, nervous diseases independent of inflammation, or any other organic affection, are still of sufficiently frequent occurrence to demand a particular attention.

"Consult," he remarks, "the treatises upon mental alienation, which is the nervous disease that furnishes the most frequent opportunities for examinations after death, and we shall find that the cases in which alterations of tissue are discoverable are the fewest in number. Even when alterations are found to exist, they are frequently of too slight a character to permit us to attribute to them the symptoms by which the disease had been characterized:—they are then to be considered as complications, or concomitant lesions; more especially as it is not uncommon to discover disorganizations to a much greater extent, without the subjects in whom they occur having been insane.

"A very little reflection upon the diseases of the nerves is sufficient to convince us, that the lesion of tissue in which they consist, is frequently inappreciable. The fact is, if these diseases depended always upon either an organic affection or upon a chronic (*latente*) inflammation of the encephalon or of its meninges, of the stomach, or any other portion of the system, they would never be recovered from in the first case, and but rarely in the second; but the greater number of the neuroses, on the contrary, disappear entirely; or rather for a time, for we are aware how liable they are to relapses, especially the vesaniæ."

In a work like the present, particularly when we consider the important task the author has proposed to accomplish by its publication, it was reasonable to expect that we should have met with a somewhat minute investigation of the physiology and pathology of the nervous system, or at least, some general views in relation to both. But, so far as it relates to the physiology of the nerves, scarcely a word is said in the whole volume, and the section upon the pathology of the neuroses (*Théories des Névroses*) contains nothing but a few naked propositions and general statements, without any attempt being made to establish their correctness by facts or argument. It is true, no doubt, that the author considered the propositions which he has here laid down, to be necessary corollaries from the facts which are supposed to have been developed by the cases previously detailed, and the remarks appended to each. Let this be as it may, we nevertheless conceive it a duty, which he owed as well to himself as to his readers, to have bestowed upon this portion of his subject a greater degree of labour; to have presented, with more of development, his peculiar views of the nature and varieties of nervous diseases, and to have compared these views with the several facts, as well physiological as pathological, by which they are supposed to be established.

The following, indeed, constitutes nearly all that the author has given to us in relation to the pathology of what he terms the purely nervous affections.

> "Every thing proves that there exist neuroses from irritation, or excess of nervous *tonicity*, and neuroses from weakness, or deficiency of nervous tone; the same as there occur sthenic and asthenic inflammations, active and passive hemorrhages, &c. The first of these neuroses, we denominate neuroses from *erethism*, and the second neuroses from *atony*. Tetanus is the prototype of the former, and paralysis of the latter. Such are the two principal elements which constitute the nervous diseases. The one is characterised by an *exaltation of sensibility*, and a derangement of the functions of the nervous system; the other, by a *diminution of sensibility*, with disorder of the nervous functions.
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> "But as nature always proceeds by insensible degrees, and never by leaps or bounds, there is found, between the most decided cases of erethism and of atony, a variety of shades more or less approaching to irritation or to debility,—so true is it, that extremes meet. In many instances it even becomes difficult, or very nearly impossible, to determine whether the energy of the nerves and brain is augmented or diminished. The disease appears then only to consist in an extraordinary *mobility* of the organs of sensation, and has, for its principal symptom, merely an aberration of the sensibility and functions of those organs. There is no doubt but that, in all such cases, there is an increase or deficiency of nervous power; but so little marked is the character of the existing affection, that frequently we can only discover its real nature from the preceding symptoms, and from the effects of remedies prescribed by way of experiment.
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> "The diagnosis of the nervous diseases presents another difficulty, which is, that although the morbid condition of the nerves termed erethism, or that denominated atony, may continue to exist during the whole course of the same neurosis, yet, it is frequently the case, that these pathological modifications succeed to, or replace each other, alternately; so that many of the neuroses, when of any continuance, present, in the phenomena by which they are characterised, a succession of excitation and of weakness."

The terms erethism and atony by which the author has designated the two opposite conditions of the nerves, upon which all purely nervous affections are supposed to depend, he has adopted, he informs us, merely from their having been already in use, and because no better ones occurred to him; he confesses, however, that neither of them conveys a correct idea of what it is intended to represent.

"What have been denominated erethism and atony of the nervous system," he remarks, "consist in two particular modifications of the nerves, unknown in their essence, and of which it is difficult to form a conception *unless we have ourselves experienced them.*"

The term atony, in particular, he has pronounced altogether improper, at least, in the greater number of the cases to which it is applied:—the nerves being never in a state of atony, strictly speaking, excepting when they have lost their sensibility, as in anesthesia and paralysis.

"The morbid condition of the nerves occurring in hypochondriasis, melancholia, &c. which is expressed by the term nervous atony, consists rather in a species of feebleness; for it is aggravated by debilitating remedies, and cured by tonics; but to this feebleness is frequently joined a very great irritability, that is to say, an *excessive sensibility*, an acute susceptibility, which exists principally after the abuse of antiphlogistics, and which should not be confounded with true nervous erethism.

"The character of the neuroses, whether of irritation or of debility, results from a difference in their causes, and in the temperaments, and individual peculiarities of the patients. A too high degree of atmospheric temperature, for example, irritates the nerves, while a humid state of the air weakens them:—individuals of a strong constitution will be affected, in general, with neuroses from erethism, while those who have a weak and delicate constitution will experience, more commonly, neuroses from atony. But when once they have occurred, these two morbid conditions may, reciprocally, produce each other; so that their alternate succession has in it nothing surprising."

On many points connected with the pathology of the nervous system, we confess, in common with the majority of the profession, our almost total ignorance; and this ignorance, we cannot say, has been in any degree removed by the labours of Dr Barras.

It is true, when we are told that nervous affections are of two varieties, the one accompanied with an increase, the other with a diminution of sensibility,—the first depending upon an irritation or excess of tonicity of the nervous system, the second upon a diminution of this tone,—there is something definite and plausible in the statement, which gives to it the appearance of truth: but when the author informs us, that in many cases of the neuroses, it is difficult to determine whether the energy of the nerves and brain is diminished or augmented,—whether there be excess or deficiency of sensibility, and that, frequently, this can only be determined by commemorative symptoms, and by attending to the effects produced by remedies administered by way of experiment, (*qu'à l'aide des tâtonnemens thérapeutiques*) we must say, that although this may possibly be the statement of a fact, it is one of which we can form no conception. But the difficulty, according to Dr B., does not stop here; for notwithstanding he has set down an exaltation of sensibility as the characteristic of the neuroses from excess of tonicity, and a diminution (*affaiblissement*) of sensibility as characterising those from deficiency of nervous tone; yet we are afterwards told, that these latter are, likewise, frequently accompanied by excessive sensibility.—It was the duty of our author, to have stated explicitly, if it were in his power, in what the *excessive sensibility* of the atonic neuroses differs from the same phenomenon when it accompanies those from erethism, and by what means we are to avoid confounding the one with the other.

The ideas of our author in relation to the pathology of the neuroses appear to us, in fact, to be extremely confused and little calculated to lead to accurate practical results; and this we apprehend is to be attributed to his not having, in the study of these affections, paid sufficient attention to the physiology of the nervous system. He has hinted at the important facts which have been added to this part of physiology by the labours of MM. Gall, Magendie, Ch. Bell, Laurencet and Foville, but we do not perceive, that the discoveries of these gentlemen have had any beneficial effect upon the opinions which he has emitted.

He has not even attempted to prove the proposition upon which the whole of his pathology of the gastralgia depends, viz. that disease may be confined strictly to the nervous tissue of a part or organ. Now we believe that this will be found seldom, if ever, to be the case; with Dr Boisseau we are of opinion, that to locate diseases in either the minute ramifications of nerves, the sanguineous, or the lymphatic capillaries which enter into the structure of an organ, is a mere pathological subtilty. We cannot conceive that one of these parts can be in a morbid state without the disease being quickly extended to the others.

What our author has termed *erethism*, that is irritation of the nerves*, would appear from his own statements to be a condition closely allied to inflammation; thus in his fifth chapter,—

> "The love of truth," he remarks, "obliges me to make a concession to the physiological physicians:—it being very certain, as we have more than once acknowledged, that neuralgic pains may be accompanied with inflammation; and the new theory of chronic intestinal diseases would not have merited all the reproaches which are now very properly brought against it, had it been applied to such cases only; but the great inconvenience and even danger of medical sys-

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* Erethism, from Εϱίϑισμα, an irritation,—strictly speaking, *an inflammation*.

tems arise from the desire to adapt them to every fact, while they apply only to a few.

"The association referred to between certain cases of neuralgia and inflammation, takes place, without doubt, in virtue of the axiom '*ubi dolor, ibi affluxus*,' and probably, also, in consequence of the administration of stimulants as remedies in the neuralgia."

After instancing a few examples of this complication, as it occurs on the exterior of the body, the author observes, that the same thing takes place, frequently, in the stomach. It is even probable, he thinks, that the inflammatory neuroses of this organ were of very frequent occurrence formerly, when stimulants were too generally resorted to in the treatment of its chronic affections.

"It is certain however," he adds, "that they are, at present, less liable to be produced by such remedies; thanks to physiological medicine, which, in place of stopping at the proper point in reference to this part of therapeutics, has gone over to the opposite extreme. We nevertheless still meet, in practice, with gastro-enteralgia of an inflammatory character, and this is not astonishing, inasmuch as certain neuroses of the digestive canal approach very nearly to inflammation of its mucous membrane, and may excite the latter without the aid of stimulating remedies. We know that these gastralgias are those from erethism,—those characterized by violent pain,—in a word, the neuralgia of the stomach and intestines.

"With respect to the gastric neuroses of an indolent character, or those attended with little pain, and in which there is only a mobility, or atony of the nerves of the primæ viæ, they are, perhaps, never accompanied with gastro-enteritis, excepting when the latter has been developed by some irritating cause, such as an immoderate employment of stimulants and cordials, of which the patients frequently make use under an idea of strengthening their stomachs; or of drastic purgatives, &c."

In the foregoing instances the gastric inflammation is supposed to be superadded to the neuralgia; but according to Dr B. cases occur in which the inflammation is the primary affection, and the nervous exaltation is only secondary. But the disease is then, he conceives, to be removed from the class of neuroses to that of inflammations, it being a gastritis complicated with nervous symptoms. This complication, we are informed, is more generally observed in irritable subjects;—hypochondriacs and hysterical females. Habitually too great, the sensibility of the stomach and intestines of such individuals passes readily to that degree which constitutes gastro-enteralgia, especially when they are excited by an attack of inflammation.

"For every focus of irritation," he observes, "and consequently of inflammation, occurring in individuals possessed of great irritability, excites in the nerves, in the neighbourhood of the irritating cause, a state of erethism and an exaltation of sensibility which may be propagated to the whole nervous system. We

> are not, however, to forget that this state of erethism and exaltation constitutes frequently the primary disease, and that it differs essentially from inflammation, notwithstanding the one may produce the other, and notwithstanding they are many times found united."

This complication of gastro-neuralgia with inflammation explains, according to our author, the real nature of those cases, in which it has been supposed that while one part of the stomach is in a state of *sur-irritation*, other portions of it are, at the same time, labouring under *ab-irritation*, or debility. The treatment of which cases has been considered highly embarrassing, in consequence of the remedies adapted to the existing inflammation augmenting the debility; while the tonics which are subsequently resorted to, with the view of removing the latter, seldom fail to revive the irritation.

Admitting the existence of pure gastric neuroses, such as the author has described; to be able to distinguish them from chronic gastro-enteritis becomes a subject of the very first importance. This distinction Dr B. has attempted to point out, by an examination of the causes of both diseases, and by contrasting their symptoms. Without following him in the detail into which he has entered, it will be sufficient for our present purpose to present a brief summary of the principal circumstances under each of these heads.

The most frequent causes of chronic inflammation of the mucous membrane of the digestive organs are, an acute inflammation of that membrane;—the abuse of alcoholic liquors, or of irritating remedies—emetics, purgatives, &c.; acrid, irritating and corrosive poisons; the presence of a foreign body in the stomach or bowels; the use of too stimulating food; irregularities of diet and all excesses of the table; ice or cold water, taken while the body is in a state of perspiration; a very cold temperature to which the individual is not accustomed; the sanguine temperament; a particular susceptibility to inflammatory affections; the suppression of habitual hemorrhages, or of the perspiration; the repercussion of a cutaneous disease; metastasis of rheumatism or gout; stimulants applied to the skin, affecting sympathetically the primæ viæ; contusions or any other species of violence inflicted upon the epigastric or hypochondriac regions.

On the contrary, the causes of the gastric neuroses are, an hereditary predisposition; a nervous, irritable and delicate temperament; a peculiar irritability of the digestive organs, either hereditary or acquired; the influence of the imagination, and particularly *the fear of being affected with gastro-enteritis, or an organic lesion of* *the stomach;* the sympathy in consequence of which the disease of one individual is excited in another; singular antipathies; a sea voyage, when the individual has never, or seldom been at sea; mental emotions, but particularly chagrin, jealousy, disappointment and paroxysms of anger; onanism, excess in venereal enjoyments, and generally, every ungoverned passion; a sedentary and studious life, profound meditation, or close application of the mind to any object, particularly immediately after meals; great heat, or a very humid state of the atmosphere; the winds of the south and west; sudden variations of temperature; an atmosphere charged with electricity; the abuse of blood-letting in the treatment of various diseases, but especially in those of the stomach and bowels; copious hemorrhages; fasting or abstinence; the too long continued use of atonic aliments, of fish, the farinacea; aqueous, diluent and mucilaginous drinks; excessive salivation; lactation; leucorrhœa; chlorosis; a state of pregnancy; the habit, in particular professions, as in that of the tailor, of bending the chest upon the abdomen, in consequence of which the epigastrium is constantly pressed inwards; the immoderate use of white wines, of coffee, tea, and other aromatics; in a word, every thing that has a tendency, either directly, or indirectly, to exalt the nervous sensibility of the stomach and intestines, and by that means to augment the susceptibility of these organs.

Passing from the causes to the *symptoms* of gastro-enteritis and of gastro-enteralgia, we find the leading phenomena of the former to be,—

Pain of the stomach, seldom intense, but constant, and always exasperated by pressure upon the epigastrium; tongue contracted, red at the edges, and furred in the centre; mouth dry and bitter; thirst; want of appetite, even disgust for food; digestion always incomplete; vomiting of the aliment, particularly of solids; in many patients acid and corrosive, or fœtid and rancid eructations; diarrhœa frequent, particularly when the inflammation has extended to the intestines; hectic fever; loss of strength with rapid emaciation, the effect of the disease itself; complexion entirely changed; progress of the complaint uniform; no intermission of the symptoms; prognostic unfavourable,—the greater number of cases terminating fatally, notwithstanding the most rational plan of treatment.

Contrast with the foregoing the principal symptoms of gastralgia, or the neuroses of the primæ viæ; which are, pain in the stomach, frequently intense, not constant, but remittent, or even intermittent; cessation of the pain upon strong pressure being applied to the abdomen* ; sensations of a strange or uncommon character within the stomach ; singular pulsations felt at the hypochondriac or epigastric regions†; tongue white and expanded ; mouth moist ; no thirst ; more or less appetite, but capricious or depraved ; digestion perfect, and, in many cases, performed with facility ; very rarely vomiting of solid food, but very frequently vomiting of liquids and of a glairy matter ; eructations insipid and inodorous ; obstinate constipation ; urine clear, passed frequently, and with a sense of burning ; ordinarily no fever ; many times, however, febrile movements of short continuance,—very rarely slow and continued fever ; preservation of strength, and very little emaciation, provided the patient has not been subjected to too severe a regimen, or to evacuations of blood ; complexion natural, or only momentarily altered or from the co-existence of some other disease ; frequently cessation of all the symptoms for days or weeks ; relapses easily excited ; in many cases hypochondriasis, or an affection of the mind, carried to its highest degree; prognostic always favourable, and a cure certain, at least under an appropriate treatment.

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* "A stomach which has been repeatedly the seat of very intense neuralgic pains, and which is still fatigued by a difficulty of digestion, flatulent distentions, &c. may preserve a species of sensibility, which is augmented by pressure; this I have myself observed ; but the attentive physician will not confound such augmentation of sensibility with pain from inflammation." Page 181, *note*.

† These pulsations are sensible to the hand, sometimes even to the eye, and may lead to the supposition that there exists an aneurism of the abdominal aorta or cœliac trunk ; especially if there is present at the same time a tumour formed by fæces or gas collected in the transverse arch of the colon. LAENNEC has himself, in conjunction with M. BAYLE, been led into a similar mistake. SCHMIDTMANN refers the pulsations alluded to, to a spasmodic oscillation of the muscular fibres of the stomach and intestines ; he thinks they may be distinguished from those of an aneurism, by their not being, like the latter, isochronous with the pulse. According to ALLAN BURNS they are occasioned by nervous contractions of the diaphragm. Dr BARRAS admits that this may be true in certain cases, but with various authorities, he conceives that the spasm which gives rise to them is frequently seated in the great arterial trunks of the abdomen ; in many cases, he states, they are isochronous with the pulse, and in hypochondriacs, and hysterical females, the carotids and other exterior arteries present the same phenomenon. According to ALBERS the nervous pulsations are to be distinguished by their sudden appearance,—their being more violent at first, and by their diminishing in intensity after a certain period. They may be produced, also, says Dr B., by encisted tumors of the abdomen, organic lesions of the pancreas, mesentery, &c. Page 183, *et seq.*

Having considered the *etiology* and *diagnosis* of the gastro-enteralgia in general, Dr B. proceeds next to describe the individual affections which he includes under this class of diseases. Of these we can only afford room for a very brief notice.

Dyspepsia. Exaltation of the sensibility of the digestive organs, without pain, constituting the first stage of many of the gastro-intestinal neuroses. Appetite variable; most generally digestion slow and difficult, accompanied with a sense of weight, uneasiness or anxiety at the epigastrium, with eructations, borborygma, flatulence, and followed by obstinate constipation. Occasionally, the above are the only morbid phenomena which are observed for a considerable time; the health of the patient being, in other respects, unaffected; most commonly, however, the affection of the stomach becoming painful, passes to the state of cardialgia or gastrodynia. In the majority of cases, the disease is not confined to the stomach, but, more or less promptly, is propagated to other parts of the system, producing there various phenomena. When this transmission has taken place, the augmentation of nervous irritability, mobility or susceptibility, becoming general, the mind of the patient participates in the disease and all the symptoms of hypochondriasis are found successively to appear. Hypochondriasis, Dr B. conceives never to exist without an affection of the encephalon. It commences, however, very frequently, in the stomach, the affection of the brain being secondary. But whenever a cerebral neurosis has become joined to that of the stomach, it reacts upon the latter, and contributes to its continuance and augmentation. Those who consider hypochondriasis as invariably a primary affection either of the digestive organs, or of the encephalon, are both equally in error. Dr B. believes that he can even go so far as to maintain that the primary seat of this vesania is occasionally in neither the stomach nor brain, but that it is sufficient for its production, in individuals predisposed to it, that the nerves of any part of the body be affected. He has seen hypochondriac symptoms occur during external neuralgia, and during pregnancy, and disappear with the removal of the neuralgia, and after delivery.

Cardialgia, *gastrodynia*, *cramp of the stomach*, *pyrosis*. These terms are nearly synonymous; they alike indicate nervous pains of the principal organ of digestion. They are subject to innumerable varieties, in relation to their intensity, the nature of the sufferings produced by them, and the periods of their recurrence. They sooner or later, if allowed to continue, produce symptoms of hypochondriasis. The pains of the stomach that so frequently accompany *fluor albus*, and which the females who experience them designate, commonly, by the expression "dragging at the stomach," constitute a symptomatic gastralgia, depending evidently upon the empire which the genital organs exercise over the stomach. That these pains are an effect of the leucorrhœa is evident, from their commencing with the latter disease, or soon after it, and disappearing when it is arrested: besides, this gastralgia, which should be named *leucorrhœal*, is ordinarily simple, that is, it is less frequently complicated with hypochondriasis than the generality of the idiopathic neuroses of the alimentary canal.

Enteralgia. Independent of the nervous affections common to the stomach and intestines, the latter may be the seat of particular neuroses, by which the former is not at all affected. Among these is the *nervous*, or *spasmodic colic*, characterised by a sense of twisting, principally around the umbilicus, or in the course of the colon, with an acute pain which is rather diminished than augmented by pressure. The pain frequently terminates within twelve or twenty-four hours, never again to return, while in other instances, like gastralgia, it is reproduced after every meal. This species of colic is unaccompanied with fever, or any symptoms of inflammation. There can be no doubt, the author, however, conceives, that nervous colic will produce *enteritis* or *peritonitis*, if it endure for a long time, is very violent, or occurs in individuals predisposed to inflammation; and hence the necessity of promptly combating it by the administration of soothing and anodyne remedies,—the only means capable of arresting its progress without contributing to the development of inflammation. Like gastralgia, enteralgia may exist under a chronic form and continue for years. Certain cases of enteralgia are accompanied by a discharge from the bowels of a watery or glareous fluid; this the author supposes to be occasioned, like similar discharges which sometimes occur in gastralgia, in consequence of the morbid state of the intestinal nerves occasioning a vicious secretion from the mucous membrane of the bowels, and also, from the difficulty there is experienced, under such circumstances, in the digestion of fluids.

Under the head of nervous colic, Dr B. includes that produced by the immoderate use of cider, beer, &c. and colica pictonum.

Nervous, or *Spasmodic Vomiting*. This frequently takes place independent of any other disease, constituting an essential or idiopathic neurosis of the stomach. The type of this vomiting we meet with, according to M. Louyer-Villermay, in certain antipathies of the stomach, which occasion it to reject particular aliments, that the appetite of the patient may even crave. Another idiopathic nervous vomiting is that produced by viewing another in the same act. *Sea sickness* Dr B. is at a loss whether to consider as essential or symptomatic;—he cannot say whether the motions of the vessel by which it is produced act immediately upon the stomach, or affect it secondarily through the medium of the brain;—he is convinced, however, that it results from a morbid condition, either primary or sympathetic, of the nerves of the former organ. He conceives that inflammation of the stomach may be caused if the vomiting endure for any length of time; but he considers this as a very uncommon occurrence, sea sickness being very seldom attended by any serious consequences. Nervous vomiting resulting from any of the preceding causes offers no difficulty in its diagnosis; but in other cases, it may be confounded with vomiting from foulness of the stomach, from gastritis, from scirrhus of the stomach, &c. and this arises from the variations to which its symptoms are subject. This species of vomiting is, however, always more easy and is attended with less pain than that from any other cause. Immediately after it has ceased the patients feel themselves perfectly alert, and able to attend to their business, or they may even satisfy the calls of their appetite, which latter is seldom impaired. Dr B. considers as nervous vomiting, that which takes place in the first period of cholera morbus; that which is occasionally met with in hypochondriasis and hysteria; that of pregnant females; and that occasioned by a lesion of those organs with which the stomach readily sympathizes, as the intestines, the brain, the kidneys, the uterus, &c. When the lesion by which the stomach is sympathetically affected is a phlegmasia, Dr B. thinks with M. Gaultier de Claubry, that the vomiting soon changes its nervous for an inflammatory character.

Malacia. *Pica*. The first is a gastric neurosis which frequently accompanies gastralgia, but which may also exist alone, that is, without pain of the stomach or other nervous phenomena. The other species of perverted appetite is observed, principally, in delicate children, in girls affected with chlorosis, and in pregnant women. In the first instance it is an idiopathic disease; the author thinks that this is also the case in the second; but in the third, it is evidently symptomatic. As individuals labouring under malacia swallow, often, great quantities of unwholesome and disgusting substances without inconvenience, Dr B. concludes, therefore, that their digestion is unimpaired, and consequently, that there cannot exist an inflammation of the stomach. In the case, also, of pica, the deprivation of appetite is unattended with bad effects, and disappears of itself, after the removal of the cause by which it has been produced, which is, also, he urges, a proof that it is unconnected with inflammation; for, he observes, genuine chronic gastritis does not disappear so easily, but frequently occasions the patient's death, a consequence never resulting from the neurosis under consideration.

Anorexia. Although the appetite is often diminished, and frequently perverted, irregular or augmented, yet it is a singular circumstance, and one of great importance in the diagnosis of the neuroses of the stomach, that we meet with few cases of gastralgia, hypochondriasis, nervous vomiting, &c. in which the desire for aliments is altogether destroyed, so long, at least, as these neuroses are uncomplicated with some other lesion. Hence idiopathic anorexia is of very rare occurrence.

Bulimia. Excepting when it results from a malconformation of the stomach, the opening of the biliary ducts into its cavity, the absence of the gall bladder, or any similar cause, bulimia is a pure gastric neurosis, and may exist either by itself, or as a symptom of gastralgia.

> "After having taught," says our author, "that an excessive appetite produces an inflammation of the gastro-intestinal mucous membrane, and that the nervous erethism, in which this appetite consists, is nothing else than the incipient stage of a gastro-enteritis, the physiological physicians nevertheless cause the persons whom they believe to be menaced, or already affected with this inflammation, to refrain from food, we had almost said allow them to die of hunger; not perceiving that their practice is here in direct contradiction to their theory: for in supposing that an excessive desire to eat will necessarily produce a gastro-enteritis, of which the erethism, upon which this desire depends, is the commencement, the first indication to fulfil, in order to prevent its occurrence, or to arrest its progress, would be, of course, to appease the inordinate appetite by a sufficient quantity of food; it being a general rule in medicine to commence the treatment of a disease by removing the cause by which it has been produced, or by which it may be kept up. In the case before us, however, the physicians alluded to pursue a directly opposite course: instead of removing the cause in order to destroy its effects, they on the contrary continue it!"

This severity in regard to diet, according to Dr B. is only proper in cases of decided inflammation of the mucous coat of the digestive organs, and then, he remarks, we have little occasion to prescribe it, as the patients have seldom or ever any desire for food. So long as they retain their appetites, it is nearly certain that their disease is merely nervous and not an inflammation; and the best means to prevent the development of the latter, is to allow them to eat with moderation. He cautions us, however, against supposing that it is always by inducing inflammation of the mucous coat of the stomach that an inordinate appetite produces injurious effects:—in general, it is only by its exasperating more and more the nervous sensibility of the digestive organ that it becomes injurious, and so little does this exasperation partake of inflammation, that the gastralgia resulting from it may endure for years, disappear, and again return, at a period more or less distant, without being attended by phlogosis of the stomach.

If the inordinate appetite which so commonly characterises the gastric neuroses were really an indication of the existence of a chronic gastro-enteritis, our author conceives that every individual who experiences a great desire for food,—convalescents, for example, in whom the appetite is, ordinarily, insatiable,—must be considered as affected, or at least strongly menaced with this inflammation: again, if the first or slightest degree of inflammation of the stomach augment the appetite, it ought to increase as the inflammation becomes more intense; but it is a well known fact that a disgust for food is the most constant symptom of a phlegmasia, or of the slightest irritation of the mucous membrane of the digestive organs, and the appetite does not return until the disease has disappeared.

"To be consistent, it is hence necessary to admit that the erethism which occasions the canine appetite is not of the same nature with the phlegmasia or irritation which produces anorexia. In fact it is impossible to believe that these two opposite symptoms can arise simply from an inflammation, more or less developed, of the mucous coat of the digestive organs. They may, however, exist in gastralgia; for those diseases which consist in an exaltation, or aberration of the nervous sensibility give rise to many anomalies, and to phenomena the most dissimilar."

Dr B. does not, however, assert that the nervous erethism which produces a devouring appetite, never gives rise to a gastro-enteritis; on the contrary, he is persuaded that it occasions it frequently, particularly when other circumstances, at the same time, concur to produce such inflammation. But he maintains, as he asserts from experience, that the erethism in question, like the gastralgia in general, of which it is only a variety, is more generally simple, and without any admixture of inflammation.

Our author here concludes his list of the individual gastro-intestinal neuroses, and after indulging in a long philippic against the doctrines of the physiological school, in the course of which we were sorry to remark one or two misrepresentations, he adds,

> "If, contrary to every species of reason, it is absolutely insisted that the name chronic gastro-enteritis be given to the gastro-enteralgia, the adjective *nervous* ought, at least, to be added, to distinguish them from genuine gastro-intestinal inflammations. But especially, should the indiscriminate employment of antiphlogistics be abstained from in their treatment; these remedies are adapted only to the first period of certain cases, and even then, to prevent any injurious effects resulting from their employment, they are to be used with much greater moderation than in inflammation. The prognosis must, also, be modified; they are not to be pronounced, in general, fatal; such an opinion, however true in the case of an actual chronic inflammation of the digestive organs, is entirely false when applied to the neuroses of these parts. I do not say that these latter affections are exempt from all danger; by the obstacles they present to digestion, and consequently to nutrition, they may occasion a loss of strength, emaciation, languor, marasmus, &c. they may, also, become dangerous in giving rise to a gastro-enteritis, to disorganization of tissue, or to melæna or hæmatemesis. But such results are rare; they occur only under the following circumstances. 1. In individuals predisposed to them. 2. When these neuroses are very violent, recur frequently, or become constitutional. 3. When they are mistaken, and improperly treated, either by stimulants, which are capable of inflaming the stomach and intestines, or by antiphlogistics, the abuse of which frequently causes the death of the patient by a total destruction of the functions of the digestive organs."

We had intended to have tested, by the diagnosis of our author, the phenomena of those diseases, which he has referred to the class of gastric neuroses, as they are presented to us in practice and in the descriptions of standard authors; but having already occupied more space than was strictly our share, we must leave this task to our readers, fully persuaded that, when they have performed it, they will find themselves under the necessity, from the author's own showing, of removing the majority of these diseases out of the class of simple neuroses. We are not to be understood, as now offering any opinion as to their real nature; we must say, however, that the author has failed in establishing, to our satisfaction, their purely nervous character: did we even profess *ultra Broussaism*, we see nothing in the work before us that should shake our faith.

Although somewhat out of place, we must be permitted to notice here Dr B.'s *speculations* as to the cause of *ramollissement*, or softening of the stomach. He thinks that this morbid change is, at least very frequently, to be ascribed to the abuse of diluent and mucilaginous drinks, by the physiological school, in the treatment of diseases of that organ. "It is certain," he remarks, "that a portion of mucous membrane placed in water soon becomes softened and decomposed." He admits, however, that there is some little difference between a portion of dead membrane and a living stomach: but, he asks, may not a period arrive when the vital properties of its tissues will be forced to give way to the continued imbibition of diluents? In support of this opinion he calls in the aid of the two physico-vital actions (*exosmosis* and *endosmosis*) described by Dr DUTROCHET*; in virtue of which it is supposed that the fluids contained in hollow organs, as well as those which surround them, penetrate and traverse their parietes.

> "Now," says our author, "this passage of fluids through organic parietes, which may be compared to an infiltration, cannot be effected without causing those parietes to undergo a species of ramollissement, which by frequent repetition may amount, finally, to a true disorganization."

We proceed next to the treatment of the gastro-enteralgia. This consists principally in a proper regimen, and the administration of "the *fixed tonics*, devoid of every stimulant principle." If there be circumstances which render the latter injurious during the first period of these affections, they are almost always necessary in the second,—other remedies are, for the most part, only palliatives; it is, we are assured, a tonic treatment, alone, seconded by a proper regimen, that will produce a radical cure of the gastric neuroses. When, however, the case is obscure, or its symptoms equivocal, so that it is difficult to determine whether it be a chronic gastro-enteritis or an enteralgia, nature is to be interrogated by the cautious administration of antiphlogistic remedies; if the symptoms be ameliorated by them, it is a proof, according to our author, that the case is one of inflammation, and *vice versa*. It is possible, however, that this proof may not be decisive, for antiphlogistics will calm, likewise, a violent nervous erethism, but then an inflammation of the stomach is threatened, and the remedies adapted to such inflammation are indicated. If, in the same manner, we commence the treatment under an impression that the case is one of atonic gastralgia, and the disease is aggravated, the remedies are to be immediately laid aside for others of a more appropriate nature.

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* See a review of Dr DUTROCHET's Recherches in our last number, p. 92.

In regard to diet; patients affected with gastralgia may indulge their appetites without danger; they are not to eat less than what they were ordinarily accustomed to, excepting when they reject their food by vomiting, which rarely happens. Those who have no appetite should confine themselves to a small quantity of aliment, unless it be immediately rejected. When the desire to eat is considerable, as in bulimia, they are to be cautious not to satisfy it to its full extent. They should be regulated by their ordinary habits, and not take more nourishment than they did when in health, whatever may be the voracity of their appetite. In all cases, they are to take their food at regular periods; but not exceeding three meals per diem. They should, particularly, avoid satisfying any desire to eat which may be experienced during the night, as such desires are, most commonly, unnatural. With respect to the kind of food proper in gastralgia, all such articles as are too debilitating or too cooling, as well as those of too exciting a nature, are to be avoided. Thus on the one hand, every species of aliment, whether vegetable or animal, in which an aqueous, fatty, mucilaginous or acid matter predominates, as well as all those which contain too much of a stimulant principle, are to be proscribed. In the gastro-enteralgia from erethism, food of the mildest nature is alone proper, such as chicken or beef broths thickened with oaten bread, crust of bread, Brussels biscuit, rice, or Indian meal; weak soups thickened with the same, with the addition of sugar and the yolk of an egg; the white meats boiled or roasted; fresh eggs; light fish; those fruits and legumina which abound in sugar and 'fecula, or at least, which contain but little water or mucilage, and for drinks during meals, water merely reddened with very old Bourdeaux or Burgundy wine; when the wine disagrees with the patient, sugared water is the most proper drink. The aliments proper for a case of atonic gastro-enteralgia, must be of a somewhat more strengthening character; the principal of these are animal jellies, osmazome, mutton or beef roasted, *légumes au jus*, wine and water, &c.

Of the remedial treatment. There are three cases in which the abstraction of blood, either general or local, will be useful or even necessary. 1. When the pain is excessive, and capable by its violence of producing gastritis. 2. When the patient is plethoric. 3. When the gastralgia has been preceded by the suppression of an habitual hemorrhage. In all others, even the moderate use of blood letting is useless, and its immoderate use always produces bad effects.

Mucilaginous drinks, particularly gum water, are invariably injurious. In certain doubtful cases they may be even regarded as a test to distinguish gastralgia from gastritis:—when under their use the disease increases, we may conclude that it is nervous, or at least that a morbid state of the nerves is chiefly concerned in its production.

When the neurosis is indolent or attended with little pain, medicines are not always required,—it is sufficient for its cure, that its cause be removed, and the patient placed under a proper regimen and moral treatment.

The best medicinal drink in gastralgia from erethism, whether painful or not, is veal or, still better, chicken water. In many cases, but particularly when there is no thirst, Dr B. directs a jelly made from the flesh of a chicken, in the quantity of a coffee spoonful at a time,—milk and water is also proper when the milk does not offend the stomach. The only vegetable ptisans at all admissible are a decoction of barley or oats, of chiendent, liquorice, rice or bread,—the two first may, however, be injurious, because of too cooling a nature. With these, certain external means are useful; as tepid bathing, or cataplasms of rice or crumb of bread, or fomentations over the stomach. The foregoing treatment is not to be too long continued, for fear of an atony of the nerves being produced. The moment it ceases to be of advantage, it should be associated with tonics; such as chicken or veal broth, with the addition of orange leaves, hops, dandelion, wild succory, germander, &c. &c., or a decoction of barley and cinchona, or of rice with the addition of a small quantity of catechu; or as a more simple prescription, we may direct the cassia lignea, the lichen islandicus, or acorns, roasted, ground, and made into a kind of coffee; Dr B. has known dyspepsias cured by the use alone of the latter. The tepid are to be laid aside for cool baths; and tonic substances, as oak bark, are to be added to the cataplasms or fomentations.

The extreme sensibility of the stomach being calmed, if medicines appear to be yet necessary, the tonics are to be continued by themselves. The best are, wild succory, rhubarb, wild valerian, gentian, carduus benedictus, colomba root, absinthium, lesser centuary, cinchona, ox-gall, the preparations of iron, Seltzer water, ice or iced water, cold or medicated baths, and to the epigastrium, a plaster of theriaca and assafœtida.

In the treatment of atonic gastro-enteralgia, we are to commence with the mildest tonics, and gradually pass to those of more power. When there is much increase of sensibility they are to be employed in conjunction with remedies of a soothing nature.

In all cases of gastro-enteralgia accompanied with augmentation of sensibility, or with pain, sedatives become necessary. Under this head the author includes ice or iced water, taken internally,—this is improper, however, in cases of decided erethism, or when a gastritis is to be feared. Among the anodynes, opium is the one the best adapted to cases of gastralgia :—for the rules to be observed in its use, and the comparative value of the other narcotics, we must refer to the work itself.

Constipation, when long continued, is to be overcome, if possible, by a proper diet ; or, this not succeeding, by injections ; but they must not be often repeated, nor must those in common use be employed. Water, rendered somewhat tonic by the addition of *cassonade* or sugar, injected of a tepid warmth in cases of atony, and cold in cases of erethism, is recommended by the author. Muriate of soda he forbids ever to be added to the injections.

When a vomiting accompanies the gastralgia, *l'Eau de Vichy*, magnesia, bicarbonate of soda, &c. may be of advantage. If there be a foulness of the stomach, neither tartar emetic, nor purgatives, will be proper ; a reduction of the diet, or even entire abstinence, will generally be sufficient to remove such symptom, if not, a cautious administration of ipecacuanha and the mild laxatives, with the exception of honey, is to be resorted to. The *poudre aérophore* of SCHMIDTMANN will also be of advantage. The composition of this powder is as follows : Carb. sodæ, one part, tartaric acid, one part and a half, white sugar, one part. To be dried by a moderate heat, well mixed, and kept in a closed vessel.

The *nux vomica* is recommended by SCHMIDTMANN in cases of neuralgia of the stomach. Dr B. restricts its employment to extreme cases, when other remedies fail. The *oxyde of bismuth*, Dr B. conceives to be a doubtful remedy. *The essential oils*, *antispasmodics*, *&c.* are always of equivocal utility, and liable to produce dangerous effects. Revulsives, externally applied, exasperate rather than remove the pain of the gastro-enteralgia. Blisters, particularly, are proper only when the attack has been preceded by the repercussion of some cutaneous disease, or of gout, or rheumatism.

The author's chapter upon *moral treatment*, *occupation*, *exercise*, *country air*, *&c.* contains, undoubtedly, many very judicious precepts; they are such, however, as will occur, at once, to the mind of every well instructed practitioner; we pass them by, therefore, without attempting an analysis.

The treatment of the gastro-enteralgia complicated with chronic gastro-enteritis, is to be effected by a proper *combination* of antiphlogistic and tonic remedies. But *combined* in such a manner that the first shall form the basis of the treatment, if the case commenced as a gastritis: " while we insist more upon the gradual and cautious employment of the second, when the neurosis has been the primary affection."

We cannot conclude our review of the work of Dr Barras without a remark or two upon the spirit in which the greater part of it has been composed, and which, we regret to say, is in the highest degree reprehensible. Notwithstanding the author in his initial paragraph admits that " the physiological doctrine has rendered important services to our science," and notwithstanding the praise he has bestowed upon the *Histoire des Phlegmasies Chroniques* of its founder, we find him, subsequently, representing the doctrine *en masse* as having retarded the progress of medicine, as destroying the sick, raising up imaginary evils to afflict those in health, as sinning against physiology, in short, as a doctrine not worth the trouble of studying. But this inconsistency is not the most serious part of the charge we bring against Dr B.—"*est huic diversum vitio vitium prope majus*"—we allude more particularly to his frequently representing the physiological physicians, not as men honestly mistaken in their medical opinions, but as entirely devoid of moral principle, as misrepresenting or concealing facts, manufacturing morbid appearances in support of their theory, and as willing to destroy their patients rather than acknowledge themselves to be mistaken in their diagnosis. These features would have deterred us from any notice of the work; but, it being one frequently referred to, by its title at least, as a complete refutation of many of the doctrines of the new school, we believed it our duty to extend a knowledge of its contents as far as the circulation of these pages will permit.

φ. R.

ARTICLE XI.—*Medico-Chirurgical Transactions, published by the Medico-Chirurgical Society, London. Part II. Vol. XIII. Longman, Rees & Co. London*, 1827.

The first part of this volume we had the satisfaction of analysing for the first volume of this Journal. The second part, the substance of which we now propose presenting to our readers, contains many valuable facts, but is certainly deficient in good essays and in original observations. Perhaps, we may be too exorbitant in our demands; but from the *London* Medico-Chirurgical Society the profession have certainly a right to look for large contributions, not only to the facts, but to the principles of medical science. The former have multiplied exceedingly, and form, it must be confessed, rudis indigestaque moles ; it is quite time for the talents of our brethren, especially in the crowded metropolitan cities of Europe, to bring order out of confusion, and thus to elevate the profession in character and usefulness. With fewer opportunities for improvement, American physicians have achieved much, and are still actively and laboriously engaged in the prosecution of the science, as well as the practice, of medicine. Facts, however, cannot be too numerous, and we shall very summarily and with few comments present those which the present volume contains.

The first paper, from DAVID GALE ARNOT, Esq., records the case of a soldier, aged 44 years, who was wounded in 1812 by a musket shot in the left side. He suffered from occasional attacks of inflammation of the lungs from exposure to wet and cold, and eventually died, Feb. 23, 1826. On dissection, the left lung was found completely hepatized; some pus was observed in the bronchiæ and trachea ; and, opposite to the third rib, a cyst was discovered, containing "a piece of iron-hoop about an inch in length and of the form of a crescent." The other lung was healthy excepting some slight pleuritic adhesions.

The case confirms what physicians and surgeons have been slow in believing, that the parenchymatous substance of even vital organs has comparatively little irritability ; their inflammatory affections being often very destructive and lasting for a long time, in the present case for fourteen years, without exciting very severe sympathetic affections.

This remark is further illustrated by the ensuing paper from Dr Rogers detailing the history of a case of injury of the head, with remarks by Mr Tyrrell.—Mark George, æt. 19, was wounded in the forehead by the bursting of a gun on the 10th of July 1825. He was able to walk some hundred yards immediately after the accident, but at length, *overpowered* by the loss of blood, he fell and shortly after became convulsed, and was totally deprived of his senses. [Were the convulsions, &c. the effect of the loss of blood, or of effusion within the cranium ?] The external wound being enlarged, exposed an orifice about the size of a crown piece in the frontal bone, just above the centre of the left superciliary ridge, through which a considerable quantity of the substance of the brain was oozing, and as much as a table-spoonful adhered to the hair and surrounding integuments. No foreign substance or spicula of bone was discovered. The wound was lightly dressed, and the patient bled to ℥xvi. with advantage. Next morning, would answer questions. Took cathartics.

July 12. Symptoms favourable. Pulse, although not exceeding forty, was tense and full. V.S. ℥xvi. Cathartic.

July 13. Pulse quicker and softer. No fever. Seems quite rational. A poultice applied to the wound; a small pledget of cerate was placed over the orifice in the bone.

July 21. Little change in the symptoms since last date. Now some irritation and febrile heat. V.S. ℥xii. Cold ablutions with vinegar and water.

July 22. Improved. Wound dressed as before. Fœtor very great, and discharge copious. Surrounding parts have a healthy aspect.

July 27. Discharge continued very abundant and symptoms very favourable until this day : has now some pain in the head—greater reluctance to answer questions ; stiffness of the lower jaw ; bowels costive.

July 28. Bowels relieved by a purgative : is better, the stiffness of the jaw gone off.

July 29. Takes more nourishment ; still some sense of choaking in the throat and difficulty in swallowing—a little inclination to drowsiness, but is perfectly sensible ; pulse sixty, soft and regular. Discharge from the wound is profuse.

August 1. No difficulty of deglutition : took a draught of port-wine and water; broth, &c.

Aug. 2. Discharge profuse and strength lessened, but by improving the diet and giving cinchona, no other unpleasant symptom followed.

Aug. 4. A foreign body was discovered in the cranial opening, but so impacted as not to be removed by the forceps.

Aug. 5. By means of a trephine elevator and a forceps, the foreign body was removed, and proved to be the iron breach-pin of the gun, three inches in length, and exactly three ounces in weight, of an irregular shape, cylindrical at one extremity, the other pointed. It had thus remained under the cranium for twenty-six days.

Aug. 16. No bad symptoms supervened; some small portions of bone been discharged. Was this day removed to his own house, about a mile distant, without injury.

Aug. 25. Cavity filling up. Able to sit up.—This boy continued to improve without any unfavourable symptoms of mind or body. Small portions of the bone exfoliated occasionally as late as the 20th of November. On the 10th of December he is reported as in good health.—The sight of the left eye is totally destroyed, but that of the right remains perfect. The smell and hearing are both perfect, and his memory and other mental faculties have not been evidently impaired. There does not appear to be any difference between the two sides of the face and head in point of sensation or power of motion.

Mr Tyrrell's observations do not increase the value of the above communication. He considers as "an extraordinary feature in the case, the trifling effect occasioned by the removal of the breach-pin," he would "have expected considerable irritation and inflammation to have followed." Upon what principle, however, is not very evident; for surgeons have always acted on the principle of removing foreign substances with the view of diminishing irritation, whether such bodies have been resident in a part for a long or short period. Mr Tyrrell's fears are analogous to those of some late pathologists who are afraid of cathartics in gastro-enteritic irritations caused or kept up by acrid ingesta: but *causa sublata*, &c.

Dr Forbes presents us with a case of inflammation of the iliac and femoral veins in a consumptive patient, with an account of the post mortem appearances. It is not very satisfactory. The patient suffered much pain in his left lower extremity, which was tense, painful and pale as in phlegmasia dolens with an enlargement of the subcutaneous veins. On dissection the cellular tissue of the whole limb was found much infiltrated with a limpid fluid, and the veins of the limb, including the external and common iliac, were greatly distended with blood. The common iliac seemed as large as the inferior vena cava; the distention of the vessel suddenly terminating where it is united with the right common iliac vein. The veins on the right side were nearly empty, and quite healthy in their appearance. The coagulated blood in the left iliac and femoral veins was of a much firmer consistence than usual, and of a more fibrous appearance. "On separating this coagulum, a thin but distinct membranous layer was seen adherent to the internal coat of the vein, and it required some force to separate them." The distention of the veins with blood and this thin membranous layer were therefore the only morbid appearances in these vessels. They are not sufficient, we think, to justify the inference, that inflammation of the veins was the *cause* of the previous pain, swelling and infiltration of the limb; for dissection revealed very slight evidences of inflammation in a complaint which had lasted for a considerable period (its actual duration is not given); and we have in the common varicose condition of the veins, and in many other cases of acute and chronic phlebitis, phlogosis of a much more severe character without the extraordinary infiltration of the cellular tissue here recorded. Moreover, what evidence does the distention of the vessel afford of inflammation? If we were left to make our own inferences from the facts of the case, we should certainly regard the distention as the cause of the inflammation, not its consequence, and should refer the painful and dropsical condition of the limb to this accumulation of blood rather than to the phlogosis of the lining membrane of the iliac veins. Phlebitis is generally characterized by a thickening and induration of the parietes of the vessel, and even where the inflammatory action is confined to the internal tunic, the quantity of blood is usually much diminished, not increased. Great attention is requisite to distinguish the causes from the consequences of disease when making inferences from post mortem appearances. A caution very requisite at the present period, when rather an undue importance is awarded to the labours of the morbid anatomist.

The next paper is entitled a "case of hydrophobia, and the appearance of the body on dissection fourteen hours after death, with some remarks on the nature and treatment of the disease. By ANTHONY TODD THOMSON, M.D. F.L.S. &c."

The case is well detailed and very interesting. We can only mention that the subject was a boy, eight years and a half old, in whom the disease appeared forty days after the patient had been bitten on the hand by a rabid cat, and twenty days after the cicatrization of the wounds made for extirpating the injured parts. The excision was performed the day after the accident. The treatment adopted by Dr THOMSON mainly consisted in the exhibition of mercurial cathartics and of the prussic acid, assisted by opiate frictions. The prussic acid had an evident influence, temporary indeed, in moderating and even in occasionally arresting the convulsive movements of the unhappy patient. Sixteen drops of this medicine were given in the course of eighteen hours. The child died at 2 o'clock A.M. November the 10th, 1826, after an illness of three days.

"*Dissection of the Body.*—The body, externally, presented no unusual appearance. There was a greater rigidity of the extremities than usual, and the body exhaled a peculiar fœtor. The first object being to examine the spinal cord, an incision was made from the nape of the neck to the loins, and the vertebræ on each side of the spinous processes being sawn through, the cervical portion and part of the dorsal portion of the medulla spinalis were exposed. The whole of the cellular membrane between the theca vertebralis and the parietes of the canal was loaded with suffused [?] blood, which in several places lay in black coagula. Six inches of the cord, from the third cervical vertebra to the fifth dorsal, being removed and the dura mater slit up, the vessels of the spinal cord were seen turgid, particularly those which accompany the dorsal nerves.

"On opening the cranium, the meningeal vessels were more turgid with blood than usual, but certainly not so great as to indicate inflammation; nor were there any bloody points seen in the divided substance of the brain, such as present themselves in states of previous great excitement of that organ. The ventricles were empty; the choroid plexus rather more turgid and darker coloured than common; and, on the base of the brain, particularly in the pia mater covering the pons Varolii and the medulla oblongata, there were evident symptoms of inflammation having affected that membrane. The lungs were gorged with dark coloured blood, but in other respects healthy; as were also the heart and pericardium. The vessels accompanying the left phrenic nerve were turgid with blood; those of the right perfectly natural.

"The larynx displayed a slight blush, which deepened on the outer surface of the epiglottis, and still more so in the inner surface. The lining membrane of the trachea was also slightly redder than usual. The pharynx shewed evident traces of inflammation; but there was no unhealthy appearance in the œsophagus. There were several patches of ecchymosis in the stomach; the liver, spleen, pancreas, and intestinal canal were apparently in a healthy state."

In the learned *remarks* which Dr THOMSON has appended, he examines the two questions "whether any change of action, sufficient to generate a poison similar to that left in the wound, takes place in the vessels of the part?" and "whether the virus be absorbed into the system immediately before the accession of the disease ?" In the absence of direct evidence in support of the affirmative of either of these questions, Dr T. inclines to the negative, as more consistent with the phenomena of the disease: and he would hazard the following conjecture, that the virus remains dormant in the part, until a certain state of habit renders the nerves in its vicinity susceptible of its influence; and this being communicated, a morbid action is begun in these nerves, and *extended to the respiratory nerves which induce the whole train of symptoms constituting the disease.* Hence, the sense of suffocation, as well as the difficulty of swallowing fluids, the gorged state of the lungs "which is a universal appearance noticed in every recorded dissection of individuals who have died of hydrophobia," the sobbings, the convulsions, &c.—may all be referred "to a morbid change in the pons Varolii, the medulla oblongata, and the medulla spinalis, communicated to those nerves which originate in these parts, and which have justly been denominated, from their functions, the respiratory nerves." In confirmation of these views, he appeals to the morbid appearances observed post mortem, by himself and by various other anatomists, some of whom he quotes, showing an inflamed or at least injected condition of the membranes or fibres of the pons Varolii and its prolongations, and of the nerves proceeding from these bodies.

The *treatment* resulting from this pathology would consist, first in diminishing excitement in the medulla oblongata and spinal cord by removing every source of irritation internal or external, and second, as soon as an intermission is effected, in promoting the tone of the system as rapidly as it can be accomplished.

"With these objects in view, I would propose to cup the patient repeatedly along the whole course of the spine, and to follow this with blisters on the same part; to exhibit large doses, say twelve or fifteen grains, of calomel, combined with tartar emetic, whilst the strength remains unimpaired; both with the view of clearing the primæ viæ, and of obtaining the sedative effect which calomel in such doses produces. I would then recommend the administration of the prussic acid, in full doses; for independent of the benefit, at least to the comfort of the patient, which I have witnessed from the use of this powerful sedative, I am induced to look for more permanent advantage from it, by knowing the success which attended its employment by professor Brera, in rachialgitis. With respect to the tonics to be administered for answering the last indication proposed, the sulphate of quinine, from its power and the small compass in which it can be given, is likely to answer better than any of the bitters or the cinchona in substance. It might be alternated with the nitrate of silver, which experience has proved to be a most powerful anti-spasmodic, as well as tonic, in chorea."

A case of extraordinary enlargement from an ovarian dropsy is next presented by Dr **Charles Thomas**. The patient Ann Payne was 52 years of age. She first observed a swelling in the left iliac region about twelve years ago, which continued to increase, so that on the 5th of July 1825 she measured, round the most prominent part, fifty-four inches, and from the extremity of the ensiform cartilage to the pubis, twenty-six inches. She was frequently tapped after the above date to the 31st of March 1826. The operation performed on that day was followed by some pain and fever. She became prostrated, and died, not from inflammation, Dr T. believes, but from the distention being too great.

Post mortem, the abdomen was found occupied with an immense tumour excepting a small space on the upper and lateral portions. There was some effusion into the peritoneal cavity; the peritoneum was about a quarter of an inch in thickness and of a cartilaginous character. The parietes of the ovarian sac varied from half an inch to an inch in thickness and contained about twenty-two quarts of coffee coloured fluid. A large mass of hydatids occupied the lower part of its cavity.

> "It was the left ovary which had acquired this enormous bulk. It was fixed to the lateral ligaments of the uterus by a pedicle of peculiar firmness, and about an inch in diameter. *It had no other attachment whatever*. The uterus and right ovary were in no respect different from their usual state in healthy women of her age."

The substance of a communication from the experienced **Merriman**, respecting the duration of pregnancy in the human female, will be found in the Quarterly Summary of the last Number of this Journal, p. 169. It is presented in a tabular form.

Mr **Norman** records the case of an extra-uterine pregnancy, in which the fœtus was delivered through an incision in the posterior part of the vagina, where the head was distinctly felt. Before delivery could be effected, the cranium had to be perforated. The umbilical cord having unfortunately ruptured during the extraction of the child, no guide existed to the seat of placenta. This body was therefore retained. The patient's strength soon yielded, and on the day after delivery she died.

Dissection confirmed the account above given. The whole of the peritoneum bore marks of inflammation, but there were no adhesions. The placenta was found attached to the right ligamentum latum, from which it was easily separated. The os uteri was situated above the pubes, as in retroversio uteri, and the vagina was so compressed that the finger could not be passed so as to feel the os tincæ from the vagina. The uterus was studded with soft tubercles, its substance thickened but not scirrhous, and its cavity was lined with a membrana decidua.

The two succeeding papers record three cases of rupture of the uterus during labour; in one the patient survived nearly eight weeks, the other two patients recovered. The two first are recorded by **William Birch**, Esq.: the third by Dr **Smith** of Maidstone.

Case 1. A female, aged 25, had two children previously; the first was delivered with the forceps, the last with the crotchet. On February the 21st 1827, she was again in labour; at 9 o'clock, the next morning, the uterus ruptured, and the patient was soon after delivered with the perforator and craniotomy forceps. Inflammation of the abdomen caused her death on the 7th of April.

Dissection exhibited adhesions of the intestines.

> "The peritoneum lining the cavity of the pelvis, and the lower part of the cavity of the abdomen, was of a dark colour, exactly resembling the pigmentum nigrum, but it had lost none of its polish, nor was it in the least more tender than natural. This appearance I had never before seen, and it gave me the notion of a stain from the long contact of blood, or of some remaining particles of blood, which the absorbents had not yet had time to remove. However, I am told by Mr **Stanley**, such an appearance of the peritoneum is every now and then met with as an effect of inflammation."

There was a contraction of the antero-posterior diameter of the pelvis; the uterus had regained its ordinary unimpregnated size; there was a longitudinal laceration, implicating the posterior part of the cervix uteri and the posterior parietes of the vagina, about one inch and a half long. The attempts at reparation were very trifling.

Case 2. Mary Wren, aged 29 years, the mother of three children; two were born alive, and one dead after a tedious labour. Labour again commenced on Sunday evening, April the 15th. The funis and a foot presented, and the head and a hand could be felt on Monday at the upper aperture of the pelvis. During a severe pain between two and three o'clock A.M. symptoms of ruptured uterus occurred, and the labour pains gradually diminished, so that soon after three o'clock they entirely ceased. The cranium was perforated with some difficulty as the head was moveable, but on attempting extraction, the head receded over the pubis, and turning the child became necessary. Owing to a contraction of the pelvis, the head could not be extracted until it was again perforated behind the ear. The placenta, being in the vagina, was immediately removed. There was no hemorrhage. Mr Birch found a rupture on the anterior part of the uterus, through which his hand passed into the abdomen and came into contact with several coils of intestine. The contracted uterus was felt, encompassed with intestines and lying at the back of the abdomen.

Some abdominal inflammation supervened, and was treated with leeches, fomentations, poultices, purgatives, and effervescing draughts, and afterwards with calomel and opium, so as to affect the gums. She had a relapse at the end of three weeks from which she completely recovered, and at the end of seven weeks occupied herself in domestic affairs. Her menses returned, and on the 6th of September she was perfectly well.

From the history of cases of ruptured uterus, Mr Birch infers, that a fatal event is more common, when the rent is on the posterior portion of the uterus; and also, that when the child cannot be immediately delivered per vias naturales, gastrotomy is much preferable to the practice, recommended by high authority, of leaving the unfortunate female to all the dangerous as well as distressing consequences inevitably resulting from the residence of the dead fœtus in the peritoneal cavity. This opinion we think defensible on every ground, especially on that of experience; for so many cases have been recorded, especially of late years, of individuals surviving the Cæsarian section, as well as gastrotomy executed not only for the removal of the fœtus, but of ovarian tumours, &c. that the natural dread of such operations has been greatly diminished, and surgeons have become, scientifically and practically, better prepared for its successful execution*.

Case 3, of ruptured uterus, successfully treated by Dr Smith, presents some other points of practical importance. Mrs H. a healthy female about 40 years of age, the mother of nine children, suffered much during her last labour, two years since, but was eventually delivered without instrumental assistance. Considerable constitutional irritation followed, with some sloughing of the vagina and bladder, and she has since been unable to retain her urine, which passes per vaginam. On the 16th of July 1825, labour again commenced during the seventh month of gestation. Pains were very severe and frequent; the os uteri dilated to the size of a crown piece but rigid, and the head presenting. Symptoms of rupture occurred about the middle of the day, and the pains of labour ceased. No part of the fœtus could now be recognized, and a rent could be distinctly felt, about two inches from the os uteri across the posterior part of the uterus, nearly three inches in length, through which two fingers readily passed. The edges of the rupture were extremely thin.

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* Vide North American Med. & Surg. Journ. Vol. I. p. 30, 235, 236, 459. Vol. II. 205, 206, 439. Vol. V. 235, 236. Vol. VI. 197.

Turning was resolved on by the attending surgeons, Mr WHATMAN, Dr SMITH and Mr CHARLES, but its execution was impracticable at the time, owing to an adhesion of part of the os uteri to the vagina under the pubis which firmly resisted the passage of the hand, —the effect probably of the inflammation and sloughing resulting from the former labour. The symptoms became more urgent, and immediate delivery was essential for the safety of the mother. It was determined therefore to make a slight incision through the indurated part of the os uteri, so as to facilitate the passage of the hand. This was effected and the child delivered by the feet, after the head was perforated at the lambdoidal suture so as to allow it to pass. The placenta soon followed. There were slight uterine pains during delivery and very little hemorrhage.

The patient had well nigh perished during the delivery, and "continued in the most alarming state for almost two hours, great nausea, unconquerable restlessness, pulse running, and at times not to be felt, quick breathing, sighing, cold extremities, and Hippocratic countenance." Brandy and other stimuli were administered, and some reaction ensued. Symptoms of abdominal inflammation supervened, and were eventually overcome by antiphlogistics, a *cold* lotion (of what is not mentioned) being employed and proving very grateful and useful to the patient. By the 22d of July, a more nourishing diet was allowed and the cinchona administered; under which treatment convalescence was confirmed, and by the 19th of August the patient was able to ride out. A report, dated 23 March 1826, says that Mrs H. continues as well as before her last confinement.

"On dislocations of the vertebræ, by W. LAWRENCE, F.R.S. Surgeon to St Bartholomew's Hospital, &c."—This is a very interesting and learned paper, embracing a review not only of cases and opinions of this comparatively rare accident, but of what has been recorded respecting inflammation, caries and anchylosis of the superior cervical vertebræ. Our analysis will be confined chiefly to the latter, after giving a summary view of the author's reading and observation on dislocations of the vertebræ. Baron **Boyer** doubts whether a luxation of the bodies of the vertebræ can occur without a fracture: **Delpech** maintains that they are never luxated: and Sir A. **Cooper** observes, that " if luxation of the spine ever occurs, it is an injury which is extremely rare." On the contrary, **Rust** refers to cases in which even the lumbar and dorsal vertebræ have been dislocated: and Mr **Bell** records a case of complete dislocation between the last dorsal and first lumbar vertebræ, with entire division of the spinal cord.

From their anatomical arrangement, the cervical vertebræ are most liable to luxation, especially of their articular processes. **Dessault** and **Boyer** mention cases: and in the museum of St Bartholomew's hospital, are preserved several specimens of vertebræ more or less displaced. Mr **Lawrence** details a case which came under his notice, showing " that a complete dislocation, both of the articular processes and body, without fracture, may occur in the cervical region of the spine." The accident occurred in a young healthy man, who, while carrying a heavy barrel on the back of his head and neck, slipped in descending some steps, and fell on the buttocks, the burthen resting on the head and upper part of the neck. The patient died early on the fourth day. On *examination* no displacement or irregularity could be discovered until the soft parts were removed; when the body of the fourth cervical vertebra was found completely detached from that of the fifth vertebra; the inferior articular processes of the former were completely dislocated forwards; the yellow ligaments, and the fibro-cartilage between the bodies of the bones, were torn through; and the antero-posterior diameter of the vertebral canal was lessened about one third;—a state of parts readily explaining the occurrence of paralysis and the consequent death of the patient from imperfect respiration.

Anchylosis of the vertebræ, the consequence of previous inflammation of the cartilages and bones, with more or less encroachment on the spinal canal, and sometimes even with dislocation, may occur—but in such cases, death is the most frequent termination. Little notice has been taken of this disease of the cervical vertebræ by English or American authors. Mr LAWRENCE gives us the outlines of a case which occurred to Mr WIGAN with a minute account of the appearances after death from a subsequent disease of the lumbar vertebræ.

At the age of five or seven, a child became the subject of an illness supposed to be hydrocephalus. After some time a swelling, obviously containing a fluid, appeared on the side of the neck and slowly increased in size. Pressure on this swelling affected the brain, producing a state of coma. The child used to move its head cautiously and slowly, supporting it with the hands at the sides. After a long continuance, the swelling and all the symptoms entirely disappeared. At no period, had there been any interruption or diminution of sensation or voluntary motion; and the recovery of health and activity was complete, the child being able to engage in the active sports suited to its age. There was nothing peculiar in the position of its head. After some time, disease of the lumbar vertebræ, with bending of the spine and abscess, came on, and the child died at the age of twelve.

Dissection exhibited the dentiform process of the second vertebra covered by the dura mater and projecting into the cranium on the right side and in front of the foramen magnum. After maceration there was exhibited an extensive displacement of the occiput, atlas and axis, and a firm consolidation of these bones in their new relative positions by the complete bony anchylosis of several articulations. The atlas is partially dislocated towards the left and thrown a little forwards and upwards; hence the right and posterior part of its bony ring diminishes the spinal canal. A considerable part of the right side of this bone has been destroyed by absorption. The axis is completely dislocated from the atlas and occiput to the right, so that its left portion intercepts about one third of the spinal canal, and the dentiform process projects by its whole length, between five and six eighths of an inch, into the cavity of the skull. The lateral displacement of this process is also great; the distance from the left anterior condyloid foramen to the middle of the basis of the dentiform process being seven-eighths of an inch, while on the right side, the distance from the right foramen to the same point is only two-eighths. No injury appears to have been sustained by the medulla oblongata passing over this projection. The bodies of the second and third vertebræ were displaced to the right. The right occipital condyle, the remains of the right transverse process of the atlas, the inferior articular plane of the axis, and the right side of the basis of the dentiform process were anchylosed so as to form one solid bony mass. The left occipital condyle is partially anchylosed with the atlas; the left articular plane of the axis, its transverse process, and that of the third vertebra were all consolidated with the partial anchylosis just mentioned. The articular processes of the axis and the third vertebra are anchylosed, the union extending on the left side as far as the basis of the spinous processes. There is no appearance of caries. The dimensions of the vertebral canal were greatly diminished. The antero-posterior diameter of the foramen magnum was in this case one inch and a half, and the greatest measurement from side to side is one inch and a quarter. The lateral diameter of the ring of the third vertebra is seven-eighths of an inch, from front to back six-eighths. The measurement of the canal between the displaced portions of the atlas and axis is half an inch from before backwards and five-eighths of an inch from side to side.

This disease of the cervical vertebræ, so rarely noticed by the English authorities, has been fully described by professor **Rust**, of Vienna, in the Salzburgh Medico-Chirurg. Journal, 1813, Vol. XIII. and afterwards in his Arthrokakologic, or Diseases of the Joints, published in 1817. He thus describes the symptoms—

"Pain in the neck, becoming more severe at night or in swallowing a large mouthful or drawing a deep breath, is the first symptom. This pain affects one side of the neck, especially when the head is moved towards the shoulder; it extends from the larynx towards the nape, and often to the scapula of the pained side. No external alteration is perceptible; but firm pressure on the region of the first and second vertebræ produces considerable pain, and thus points out the seat of the disease. The difficulty of swallowing and breathing, and the hoarseness increase, alternating with pain in the neck, which seems to fix about the back of the head, and becomes intolerable on moving that part. The head sinks towards one shoulder, the face being turned a little down; for in general the articulations are affected on one side only, and that was the left in seven out of nine examinations after death. If both sides are affected, the head will incline directly forwards. In this state, things continue for several weeks or months; and before worse symptoms come on, there is often apparent improvement, freer motion, and more natural situation of the head. But, the uneasiness in speaking and swallowing returns, the pain becomes more severe and extensive, the head falls a little backwards and sinks towards the opposite side. The patient feels as if the head were too heavy, and he carefully supports it with his hands, when he moves from the sitting to the lying position, or vice versâ. This may be considered as a pathognomonic symptom of the affection. Another symptom, which, at this period, shows the true nature of the disease, is a peculiar expression of pain in the countenance, which, combined with the position and stiffness of the head, constitutes so characteristic an assemblage of appearances, that it is enough to have seen it once, in order to recognize it immediately. This look of the patient, which RUST has endeavoured to represent in an engraving, consists especially in a general alteration of the features, with a heavy motion of the eyes, and a dull melancholy expression of internal painful sensations. More active indications of severe suffering are observed whenever the head is moved.

"In the further progress of the case, noise is heard in the head; deafness, giddiness, cramps, and convulsions, partial paralysis, particularly of the upper limbs, loss of voice, purulent expectoration and hectic symptoms supervene. Generally no external change is observable, either in the neck or in the nape; and RUST observed, in one case only, swelling of the affected side, which broke and left fistulous ulcers. But the slightest pressure in the region of the three upper vertebræ is acutely painful, and sometimes, in the advanced period of the disease, a grating of rough surfaces is distinctly perceptible when the head is turned. The patient may continue for months in this helpless and painful state, and then dies, either from exhaustion and debility, or, which is more frequent, suddenly and unexpectedly."

This description of the disease by RUST, Mr LAWRENCE considers as indicating "originally ulceration of the cartilages proceeding to the destruction of the ligaments and to caries of the bones, and extending in various shapes and degrees to the neighbouring important parts." The state of the parts, examined post mortem, of course, varies greatly. REILL mentions that in one example "the occipital condyles, the bodies of all the cervical and of the four upper dorsal vertebræ were found in a softened state, while the articular surfaces of the condyles and the two first vertebræ were partly destroyed." The appearances of the diseased parts in a more advanced state are described by RUST.

"I have always found, in a larger or smaller quantity, matter between the pharynx and vertebral column; it has sometimes been of a good kind, sometimes sanious. The pharynx is more or less ulcerated, and the periosteum and ligaments of the first vertebræ are destroyed. I have always found the original seat of disease in the articulations of the occiput and atlas, and of the atlas and axis; caries has attacked the occipital condyles, more frequently the left than the right, the atlas and the dentiform process. Where death has come on suddenly, the diseased dentiform process has been entirely broken off, or blood has been effused from ulceration of the vertebral artery, or pus has been found in the chest. The neighbouring important organs may be more or less seriously affected. Serous effusion is sometimes found between the dura mater and the arachnoidea, and in the ventricles. In the vicinity of the foramen magnum, where the space for lodging the spinal cord is sometimes much contracted, the dura mater is found livid, ulcerated and sloughy, and the neighbouring parts of the brain and medulla more or less altered in appearance. I have placed several preparations, illustrating this affection, in the pathological museum of the Vienna general hospital.

"In some cases," says Mr LAWRENCE, "the disease appears to be confined to the articular surfaces. After destroying the cartilages and ligaments, and producing more or less extensive displacement of the occiput and two first vertebræ, it stops, and the process of reparation commences, ending in bony anchylosis, by which the displaced parts are firmly fixed in their new position. Yet, in these more favourable instances, there is great suffering during the inflammatory stage; the extension of active disturbance to the dura mater, and the neighbouring contents of the cranium and vertebral canal, may cause dangerous and alarming symptoms, while chronic abscess may be formed externally as in the case which I have related."

In estimating the degree of pressure sustained by the spinal marrow, allowance, says Mr L., must be made for the fact that this medullary cylinder does not occupy the whole canal of the spine; hence dislocations, at least spontaneous dislocations, may occur, as in the case above alluded to, in which no pressure is experienced by the spinal marrow, and of course no paralysis results.

Patients occasionally recover from caries of the upper cervical vertebræ. Rust has seen one case of recovery, although he intimates that all the cases under his care terminated unfavourably. Mr Lawrence, however, has collected a number of facts from Reill, Wynpersse, Daubenton and others, and refers to several specimens in the museum of St Bartholomew's Hospital, showing that anchylosis of the superior cervical vertebræ has not unfrequently occurred; and that the patients have eventually perished from other diseases.

Mr Lawrence recommends no peculiar treatment for these inflammations. Rest, assisted by the common antiphlogistic remedies, should be steadily persevered in, as in similar affections of other portions of the spine.

We have been induced to give this extensive analysis of Mr L.'s paper, as sufficient opportunities are not afforded in this country for a proper investigation of this comparatively rare complaint, which there is reason to believe is occasionally observed.

Two papers are presented to us on the propriety of destroying nævi materni by strangulation; the first is from Mr Lawrence, the second from Mr Anthony White. To this last gentleman credit is given by Mr Lawrence for introducing this practice several years since; it was suggested also by Mr John Bell and by Mr Wardrop. The advantages to be derived from the ligature, single or double according to the size of the tumour, relate chiefly to the danger of hemorrhage, by which patients have been destroyed when the knife has been resorted to. See Mr Wardrop's case in the Medico-Chirurgical Transactions, Vol. IX. p. 203. The subsequent cicatrix is said also to be smaller than when excision is practised, and although much pain and irritation are excited by tying the ligature so as to strangulate the tumour, yet no bad consequences have resulted, and these symptoms immediately disappear when the ligature is removed at the expiration of twenty-four or thirty-six hours. The ligature may safely be resorted to in cases where the tumour is so large and the vessels so dilated that the extirpation by the knife would be wholly inadmissible. This method has proved more successful, according to Mr LAWRENCE, than any of the measures formerly suggested; than the caustic, lunar or vegetable, as proposed by Mr WARDROP; than cold, astringents or pressure; and more effectual also than tying the supplying arteries or even taking up the main trunk of the supplying vessels, an operation which, although it has been occasionally successful, promises very little for the radical treatment of these "erectile tumours," so named by DUPUYTREN. Interesting cases are given by Mr WHITE and Mr LAWRENCE, which we need not detail, especially as we have already noticed this operation in the fourth volume of this Journal, p. 425.

On the use of sulphate of copper in chronic diarrhœa, by JOHN ELLIOTSON, M.D.—We have, in this communication, the history of several cases of chronic diarrhœa and dysentery, in which the sulphate of copper, in doses of half a grain to one grain and a half, usually combined with half a grain or a grain of opium, was very effectual in restoring patients to health where all the usual practice had failed and when no other apparent hope remained for the sufferer. The opium was given to prevent nausea and irritation from the cuprous salt; that it however was not the efficient agent was shown by comparative trials of the two articles, and especially by the fact that the opium alone, or in combination with astringents, had previously failed in the same cases in which the pills of opium and copper succeeded. Several of the cases were of long standing, with every indication of great derangement of the chylopoietic secretions; the stools being often very copious and numerous, like a mixture of lime and water, frothy and of a sour smell; even when of some consistence, still pale, and when even they become figured and dark, lighter than in health, and soon becoming frothy and white. In other cases, the discharges were bloody or mucous, attended with pain and scalding. The sulphate had the effect of altering the character of the secretions, so that they became more natural in consistence and colour, often quite bilious, and at the same time, the appearance and strength of the patient rapidly improved, and perfect health was eventually obtained in almost every case.

Dr Elliotson has hardly alluded to the pathology of chronic diarrhœa and dysentery, or to the modus operandi of his proposed remedy. On the former point, we are much indebted to the French school of medicine, whose laborious investigations of late years have shed great light on these subjects and entirely overthrown all preconceived hypotheses on the subject of passive exhalation and chronic debility in these wasting and exhausting diseases. Ocular demonstration is daily afforded to those who will look, that inflammation of the mucous tissue of the small or large intestines, in its various stages from simple erythema to complete ulceration and gangrene, constitutes the disease under which the patient's strength is so gradually and so frequently exhausted. The idea of imparting strength to an individual thus circumstanced, merely by exhibiting tonics and astringents, and especially solid nutritious aliment, is as absurd in theory as it is injurious in practice. So long as the strength of the patient is adequate, the common antiphlogistic measures, leeches, cups, fomentations, blisters and other revulsives, assisted by cold mucilaginous enemata and a diet strictly farinaceous, will answer if steadily pursued. When however strength is rapidly failing and when structural derangement is occurring, a more nourishing diet is demanded, but this should always be of a pulpy or liquid consistence; solid food, rapidly driven through the irritated alimentary canal, operates invariably as a *mechanical irritant*, aggravating all the symptoms, and bringing on relapses even in patients quite convalescent. Without a liquid diet, no medicine can be of much avail. This practice may then be assisted by those alterative measures which have been found so useful in other chronic *mucous inflammations*, as of the eye, urethra, &c. Hence the advantage derived in chronic dysentery from Moseley's vitriolic solution (of alum and sulphate of zinc), from pills of acetate of lead as recommended by Drs Harlan and Coates, from the nitrate of silver as lately advised by Dr Johnson in morbid irritability of the stomach and bowels, and no doubt also from the sulphate of copper as now proposed on experiment by Dr Elliotson. Clinical experience demonstrates, that such stimuli, long used advantageously in external inflammations, do not aggravate the phlogosis of the internal mucous tissues when judiciously exhibited*. To the same test, we may appeal for confirmation of the practice of exhibiting suitable cathartics in almost every stage of bowel complaints. The disciples of the BROUSSAIS school will exclaim, " What! exhibit irritants when the disease is irritation!—will you add fuel to the devouring flame?" Cathartics of every kind may be termed irritants—but most of them are irritants of that peculiar character, that in almost every instance secretion is excited, so that large quantities of fluid are poured out by the inflamed vessels, and thus their excitement is reduced precisely on the same principles that a hot poultice relieves a phlegmonous inflammation. Purgatives are useful in bowel complaints, not only by exciting capillary secretion, but especially by removing the acrid, solid and fluid contents of the bowels,—often the cause of their phlogosis and always aggravating it however excited. This is an indication almost wholly neglected by the BROUSSAIS school, who tremble at the idea of the irritation from an ounce of salts, and yet allow the undigested fermenting or putrefying food and the scalding secretions of diseased tissues to remain, for days or weeks, in contact with the irritated mucous membrane. The irritation of the muscular coat of the intestines, excited by a cathartic, may be injurious, but experience shows that the injury must be very trifling and can form no objection to its employment when the above mentioned indications are to be fulfilled, especially as the regular peristaltic motion thus excited is much less painful than the irregular spasmodic contractions from acrid ingesta, bile, mucus, &c.

We have been led to these observations by two considerations; First, That the establishment of correct principles in medicine, principles legitimately deduced from the laws of the economy and from the modus operandi of the causes and remedies of disease, is all important, and will often, as in the present case, lead to scientific and successful practice. A second motive for this digression is to protest against hasty assumptions and illegitimate conclusions which are opposed to careful experience. As may be readily observed, the opinions of the Editors of this Journal are different on many points,—a reason why they were appointed; but they are all *eclectics*, and not devoted to any school,—they wish and labour to test every opinion by clinical observation, and dread, as much as any lover of empiricism, the terrible consequences of unphilosophical system-making. They nevertheless are advocates for principles in medicine, and believe that more good than harm has resulted from the efforts even of those great minds whose imaginations have occasionally led them astray from true philosophy. Many who are too indolent to collect and collate facts, rail at theory as the bane of medicine, forgetting that theory constitutes the identity of medical science, without which empiricism—ignorant empiricism would reign triumphant; from which may we and our patients be preserved by a kind Providence.

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* Influenced by the reasoning in the text, the writer of this analysis has resorted to injections of a solution of sulphate of zinc (grs. x.—xx. to ℥viii. of water) in these chronic inflammations, after the violence of the disease has been subdued and the symptoms of general excitement disappeared. The practice has been very successful, and on comparative trials has appeared to him more useful than when the same article has been administered by the mouth, as no nausea is excited, and the application is made directly to the inflamed surface. At some future period, cases of its efficacy may be presented to the public.

In a postscript to his paper, Dr Elliotson states summarily the result of his experience with *quinine* in intermittents, and the *subcarbonate of iron* in chorea. The advantages of the former he fully confirms, and adds that he has with complete success given the quinine in intermittents, when complicated "with so much inflammation in the abdomen, chest or head, as to require venesection, some with dropsy, some with a chronic disease of the lungs or liver; *but every one was cured*." He has been convinced that the best practice is to give ten grains as soon as the paroxysm is over, which will often be sufficient for preventing a return, but it is sometimes necessary, in addition to these ten grains after the fit, to make the whole quantity in the twenty-four hours amount to a scruple, or half a drachm, by small doses also, every six or eight hours.

The subcarbonate of iron in large doses was recommended by Dr Elliotson in the former part of this volume of the Transactions as very successful in *chorea*, eight cases of which he detailed (Vide N. Am. Med. & Surg. Journ. Vol. I. p. 455). He has since attended nine cases of the disease, *every one* of which yielded to the remedy. The time usually required varied from six to ten weeks; one was cured in four, and one in not fewer than twelve.—He has also found it useful in large doses, in chronic ulcers, and in a genuine case of traumatic tetanus, and would recommend its trial not only in tetanus, but also in hydrophobia, in doses of half an ounce every two hours.

Dr Elliotson extols *acupuncture* very highly, chiefly in rheumatism of fleshy parts,—and the more so, as the disease is less inflammatory. "The effects are often magical. Of forty-two cases, taken in succession from the hospital books, thirty were cured."

Mr William Wallace, surgeon to the infirmary for diseases of the skin at Dublin, describes a peculiar eruption of a fungous character, to which he gives the name of "*morula*," from *morus* a mulberry, from its resemblance to this fruit. A minute description, for which we have no space, and a plate would be necessary to convey an idea of this disease. Mr W. has met with the complaint chiefly in the vendors of old clothes; it appears to be contagious,—is not of syphilitic origin, and is very readily cured by mercury, internally or externally employed.

Local applications, not containing mercury, have little influence over the disease.

The three succeeding papers relate to injuries of the hip joint, the diagnosis of which is so difficult, even to experienced surgeons, that every observation on the living or dead subject demands attention.

The first paper, by Mr George Langstaff, mainly consists of a description of various morbid preparations in his possession illustrating the kind of union which results after fracture of the cervix femoris within the capsule. Whether a bony union is ever possible has been denied by many surgeons; that it is a rare occurrence has been admitted by all. The facts stated by Mr L. are interesting and valuable, although he details no case in which a complete bony union had occurred. In one, however, the union was principally osseous and partly cartilaginous; the neck of the bone was greatly shortened by absorption; the capsular ligament was immensely thickened, and embraced the joint very closely; the cartilaginous covering of the head of the bone and acetabulum had suffered partial absorption; and the internal surface of the capsular ligament was coated with lymph. The patient was a woman about 50 years old, had been confined to bed nearly 12 months, and during the remainder of her life, which was ten years, walked on crutches.—In other cases, Mr L. discovered various alterations of structure indicative of the degree of power of reparation possessed by the injured parts. In all, the neck of the bone was shortened; in most, the union was effected by short ligamentous fibres,—sometimes there was a partial joint; and in very many cases, the capsular ligament was greatly thickened, often covered internally by lymph or even cartilage, and in two cases at least, if we understand the description, portions of the capsular ligament became osseous, thus increasing the firmness of the injured parts. Mr LANGSTAFF has long been satisfied of the possibility of bony union in fractures within the capsule, but "why the ossific union of fractures within the capsule is of more rare occurrence than in fractures exterior to that ligament, is still a pathological problem of difficult solution." We, however, perceive no difficulty; for the upper fragment, when the fracture is above the insertion of the capsular ligament, is so sparingly supplied with nervous influence and blood (the round ligament and synovial membrane being almost the only media of communication) that it can contribute very little, if any thing, to the process of regeneration: in the language of Mr HUNTER, it merely "accepts of the union" offered by the inferior fragment.

The second paper, by Mr EDWARD STANLEY, of St Bartholomew's Hospital, records some cases of fracture of the trochanter and of the root of the cervix, in which the diagnosis was difficult, owing to the fragment of the trochanter being drawn to the situation occupied by the head of the os femoris in some dislocations at the hip joint. The difficulty is often increased by the fact that the foot in such accidents is occasionally turned inwards, as in dislocations, and crepitus is not always to be detected. Why the limb should ever be inverted in such cases is not easily to be determined; it probably depends on the manner in which the accident has occurred. Mr S. records, also, a case in which there was a fracture of the neck within the capsule without any rupture of the synovial and fibrous covering. The patient died, from an inflammation of the intestines, in the fifth week from the date of the accident. There was a slight effusion of blood under the membranes and in the tract of the fracture. Such cases would no doubt unite by bone.

In the third paper, on accidents of the hip joint, Dr BRULATOUR of Bourdeaux gives the particulars of an accident which occurred, on the 20th of March, to Dr JAMES, an English physician, at that place. Dr J. was thrown from his horse and manifested the principal signs of fracture of the neck of the femur. He was confined to his bed for two months; during the whole of which period, permanent extension was preserved. At the end of the third month, his crutches were put aside, and he walked with only the assistance of a cane. In August, he could walk without any assistance, and subsequently recovered the full use of his limb. He, however, died in December from a violent attack of hæmatemesis.

On examination of the hip joint, there were observed, 1. The capsule a little thickened; 2. The cotyloid cavity sound; 3. The interarticular ligament in a natural state; 4. The neck of the femur shortened; 5. An unequal line surrounded the neck, denoting the direction of the fracture; 6. At the bottom of the head of the femur, and at the external and posterior part, considerable bony deposit had taken place. A section of the bone was made in a line drawn from the centre of the head of the femur to the bottom of the great trochanter, so as perfectly to expose the callus. The line of bone indicated by the callus denoted also that the bottom of the head of the femur had been broken at its superior and posterior part.

On paraplegia, by H. Earle, Esq. Surgeon to St Bartholomew's Hospital, &c. Nervous diseases are the most intricate and the most unmanageable affections to which the human system is subjected. Their pathology can only be revealed by investigations primarily illustrating the physiology of the brain and its dependences, and by a careful comparison of facts from the morbid anatomist. Unfortunately, in our opinion, the past and the present medical philosophers have regarded too exclusively the vascular system in nervous diseases,—these last being regarded as secondary, the result of sanguine irritation. Hence the discovery of vascular turgescence, of effusion, or of structural alteration, has been deemed by many an adequate explanation of the phenomena during life, and, of course, a sure basis for therapeutical measures; forgetting that these nervous complaints, in all their intensity, have often existed without any evidence, ante seu post mortem, of vascular irritation, are seldom in proportion to the degree of sanguine disturbance, and, moreover, that such vascular affections are very frequently detected without any corresponding nervous disease. But little examination of bodies will, we think, confirm these facts. The nervous system *should be studied by itself* physiologically and pathologically; for on it are impressions primarily made, and disorders originally induced: the active or passive disturbance, excited or depressed state of the circulation is very generally secondary, and although demanding much attention is comparatively unimportant. These opinions we have long entertained, and they daily derive support from reading and observation. An analysis of the very interesting and valuable cases which Mr **Earle** has presented in this paper would confirm, we believe, the above views, but we have no space for further remarks or even for a summary of his facts. We shall present a few extracts illustrative of the opinions which he has deduced from the cases and dissections presented.

By paraplegia he understands that species of palsy in which both sides of the body, partially or generally, are affected, in opposition to hemiplegia, in which the affection is confined to one side. Paraplegia is not alone dependent on disease of the spine, or its contents, but will occasionally be found in chronic diseases of the brain and its membranes. The diagnosis is not always easy, but may often be tolerably correct, if a close attention be paid to many circumstances.

"Paraplegia, dependent on disease in the brain, generally occurs at the middle or more advanced period of life than is usual in diseases of the bodies of the vertebræ, or their intervening fibro-cartilages. Its progress is more rapid than the slow insidious approach of symptoms from the latter diseases; the affection is more general, occasioning more or less paralysis of the upper and lower extremities, and this will often take place in a very few days from the occurrence of the complaint. The disease happens much more frequently in men than women. The gait of persons suffering from cerebral affection is peculiar, and very different from that attendant on affections of the spine. It very nearly resembles the vacillating steps of a drunkard.—Such paralytic persons are incapable of walking in a direct line; the limbs are loose, and thrown forward with an exertion of the whole body; there is a great consciousness of feebleness in walking, and the greatest difficulty in turning round. The appearance of the eyes often much resembles those of a drunkard, particularly when the patient is at all excited or anxious. The above analogy to the staggering steps of intoxication is readily understood, if we consider, that it is the temporary disturbance from the congestion of its blood vessels, that deprives the drunkard of the power of directing his steps, and for a time induces a state bearing the closest resemblance to paraplegia.

"Sensation is more impaired than in spinal affections, when it will often remain perfect after a total loss of the locomotive powers. This impaired sensation is often peculiar, imparting an idea of some foreign body, as a leather glove or stocking being interposed. The patient appears to feel, if I may use the expression, through a false medium; the limbs are more wasted and flabby, without any spasmodic rigidity of the muscles, which so often occurs in affections of the spine. Although often accompanied with a torpid state of the bowels, aggravated, no doubt, by the impaired muscular power of the abdominal parietes, there has not, in any instance I have witnessed, been any train of gastric symptoms, similar to those which so constantly attend affections of the spine, especially of the dorsal region. In some instances, there is the additional confirmation of an impaired state of some of the external senses, accompanied with vertigo, a sense of weight on the head, and a general disturbance of the cerebral functions. As disease advances, the power of the brain in transmitting its influence to the extremities becomes more and more circumscribed.—Thus I have known a tubercular affection of the pia mater, in the first instance, cause a numbness and loss of feeling in the feet, which has gradually extended until all four extremities were completely paralysed, and the muscles concerned in respiration at length refusing their office, death ensued."

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"Whenever disease has proceeded so far, one or more of the mental faculties has, in almost every instance, suffered, and no reasonable doubt can, in such cases, be entertained respecting the real seat of the disease. It is in slighter and more chronic cases that it is often difficult to form a correct opinion; yet to establish a correct diagnosis in such cases is of the utmost importance, both with respect to the prognosis we should form of the probable termination of the case, and with reference to the proper treatment to be adopted, that we may not subject the individuals to useless suffering from the application of caustic issues and setons to the spine, and the disappointment which would follow their obvious inutility in such instances. In several of these cases, attention to the following circumstance has materially assisted me in forming a correct judgment; I am therefore induced to lay considerable stress upon it, although to some persons the opinion drawn from it may appear somewhat speculative. It is well known, that when a nerve is stimulated or injured in any part of its course, the painful sensation is referred, by the percipient mind, to the sentient extremity of the nerve; the familiar instance of the pain referred to the extremity of an amputated limb, may be adduced in proof of this. The exact reverse of this takes place, when there is a partial paralysis arising from morbid affection of the cerebral organs. Here the centre of the sensorial functions being impaired, it appears to be incapable of transmitting its influence to the extreme parts of the body, and thus the feet and hands gradually lose their sensation or power of motion, or both; and in such cases if the nerves supplying the limbs be irritated, *they will convey the impression of such injury only part of the distance down the limb, about as far as the commencement of the paralytic affection.* I have repeatedly examined cases of paraplegia from affection of the spine, and in no one instance have met with the same phenomenon, which I have therefore been induced to consider as diagnostic of the paralytic affection being dependent on disease of the brain or its membranes, which opinion has, in several instances, been confirmed by examinations after death, in which both brain and spinal cord have been carefully investigated."

Mr E. cautions us against a fruitful source of error, relative to the seat of the disease in paraplegia, arising from the incurvated state of the spine. This spinal curve in affections of the brain implicates the whole column, which resembles a half-hoop, in consequence of the debility of the muscles of the back, which are no longer capable of maintaining the erect posture. The curve will immediately and spontaneously disappear in a horizontal position. In diseases of the spine, the curve is more angular and defined; and distressing symptoms often follow attempts at restoration.

This valuable communication is closed by a detail of cases—1. Of paraplegia dependent on cerebral affection alone; 2. Of paralysis from diseases of the spine; And lastly, where disease existed in the same individuals in both brain and spinal marrow. They are all worthy of record and examination.

Observations on the necessity and method of further investigating the distinctions between syphilis and other varieties of venereal disease, by R. Welbank, Esq.

This paper, manifesting some talent and observation, does not, however, admit of analysis without making very copious extracts, for which we have no room. The author would seem willing to refer the various primary and secondary affections from impure coitus to two or three distinct poisons, ascribing their innumerable varieties to the tissue concerned, and especially to the peculiarities of the patient's constitution. The two most important venereal affections are syphilis and phagedæna; the former generally curable by mercury and often demanding its employment, the latter seldom benefited by this mineral. To these he would add the venerola vulgaris of Mr Evans, seldom demanding active treatment.—To those who are investigating the varieties of syphilis, the paper of Mr Welbank will afford many useful hints and cases.

The concluding paper in the volume gives a detailed account of a successful case of amputation at the hip joint, executed by Mr John Orton under very unfavourable circumstances. The patient had been long suffering from caries and abscesses of the knee joint and thigh, some of the sinuses extending to the buttocks. He was excessively reduced by hectic fever, and was also suffering from a "bed-sore" on the sacrum with necrosis of this bone and the coccyx. The operation was performed June 10th, 1824. In August he was able to move about on crutches, and recovered completely, remaining well in January 1826. Much credit is due to his surgeon for the assiduous and scientific attention manifested in the after treatment, to which the unexpected recovery of the patient is to be mainly attributed. Any man with common dexterity and courage can remove a limb; few, however, can so combine science with art,—the constitutional with local treatment,—as to afford the patient the best opportunity of escape, confer permanent honour on their profession, and facilitate the true interests of humanity.—In our estimation, Mr ORTON has gained more credit by his after treatment than even by his well executed operation.

We cannot dismiss the volume before us without a passing tribute to the lithographic plates executed with much neatness by J. PERRY.

Medical Literature.

ARTICLE XII.—*On the Reciprocal Circulation of Blood between the Mother and Fœtus. By* M. MERY.—*With Preliminary Remarks. By* JOHN REDMAN COXE, M.D. *Professor of Materia Medica in the University of Pennsylvania.*

IN the July number of the North American Medical and Surgical Journal, page 168, is a very interesting abstract of a paper of BIANCINI, " on the communication between the blood-vessels of the uterus and placenta." In this instance, the blood-vessels of a woman who died in child-bed were injected, *the placenta being undetached.* The injection, on dissection, was found to have passed *into the vessels of the chorion and amnios*, the tortuous arteries of the uterus having penetrated the tissue of the placenta, &c. Other analogous facts are adverted to, relating to the human, as well as to the brute subject. How the opponents of this opinion will overcome the dilemma in which the above fact places them, we know not; but presume, as heretofore, they will boldly deny the fact, or distrust its accuracy*.

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* The demonstration of this fact so long ago as 1752, by the same means of injection, rests on the authority of the highly celebrated M. LE CAT. This communication was made to the Royal Academy of Sciences, Belles Lettres and Arts at Rouen, on the 11th of July of the above year. A slight notice of it appears in the Recueil Périodique d'Observations de Médecine, Chirurgie, Pharmacie, &c. 1754, Vol. I. p. 253, in the following words, " Le même (M. LE CAT) ayant démontré à l'Académie, le 11 Juillet 1752, la communication des vaisseaux sanguins du fœtus et de la mere, dans une placenta attaché à un utérus et injecté, il a trouvé cette année *deux* occasions pareilles de répéter cette expérience, la premiere fois devant les commissaires de l'Academie, et la seconde, en particulier, avec un succès égal a celui de 1752."

It must be more than simple denial, that shall be permitted to overthrow authority like this; or even to explain the experiments away by sophistry or plausible argumentation.

It would indeed seem, that attachment to hypothesis has always led to such uncandid measures; not on the point in question only, but on many others; hence too it is, that numerous and well attested facts in ancient writers have been completely overlooked or forgotten in the present day. It would seem as if all vestiges of *ancient* knowledge, either as to fact or theory, are now considered nugatory; at least they are rarely brought forward, unless they should chance to uphold some favourite opinion, probably advanced as new in medical science; for dogmas and hypotheses now sprout forth like mushrooms;—and fortunately, if rapid in their growth, they have, within the last few years, been equally so in decay. It has been said by a wise man that there is nothing new under the sun; certain it is, there are few of the *so esteemed new* doctrines in physiology, pathology or other parts of medical science, but can be clearly found to have formerly prevailed! If the publication of that cumbrous work, the "Dictionnaire des Sciences Médicales," in sixty volumes has been attended with the disadvantage of leading our writers (both *of* books and *in* journals) to quote largely from or to refer to authors whose works they have never seen and rarely heard of; it has so far been useful, that it has elicited the names and productions of many illustrious medical worthies, who may possibly be hereafter deemed worthy of recognition by our schools; and even recommended as teeming with useful and important truths. We speak of GALEN and HIPPOCRATES with apparent candour and approbation: we still call the latter the father of medicine, &c. And yet few think them worthy of the slightest personal acquaintance. In this respect, we are to blame; if great as they are acknowledged, they deserve to be investigated; and if they are not, the sooner this is demonstrated the better, that they may not be ever again quoted as authority.

What is above said may be considered as irrelevant to the object mentioned at the commencement of these remarks: and yet it is closely connected therewith, inasmuch as it is this total neglect of the older writings that has precluded us from knowing that facts as strong as those of BIANCINI have long ago been given to the world, in order to settle a question then agitated amongst physiologists with equal warmth as latterly.

In BERRYAT'S "Mémoires ou Collections de Piéces Académiques," Vol. II. p. 624, we have a communication from M. MERY to the Royal Academy of Sciences at Paris, in 1708, which I should have supposed, would, *if known*, have been sufficient to settle the dispute, or that at least it would have been employed in the later discussions of the question. It is scarcely necessary to request the publication of the whole communication. The work referred to is so rarely to be met with, that I presume M. Mery's paper to the majority of readers, will prove in truth a *new* one; and I regard it as a favour to the profession to bring it to their knowledge. The problem, again verified by the experiments of Biancini, it will be seen, was ably and fully solved by M. Mery a hundred and twenty years ago; and by a different process, tending thereby to render the truth more impressive. The character of that gentleman, as an accurate observer and accomplished surgeon, must be known to many. It will be perceived that the question had been one of long standing in the schools, but we trust is now definitively settled. The second problem, which M. Mery also resolves by reference to the first, viz. the mode of nourishment of the fœtus, is not essential to the subject under consideration: it is therefore omitted.

M. Mery's communication is thus headed.

A Problem in Anatomy: to wit,—Whether, during pregnancy, there exists a reciprocal circulation of blood between the mother and child.

The opinions of anatomists, says M. Mery, are at present strongly divided on the question. On the one side it is affirmed that the blood of the mother *does not pass* into the fœtus, nor that of the fœtus into the mother; and that the only nourishment it receives from the mother is chyle furnished by the glands of the uterus.

On the other side they are persuaded, *that there is* a reciprocal circulation between the two; and that the infant is nourished only by the maternal blood, which is sent to it from the uterine arteries through the umbilical vein; and it is moreover denied that any glands exist in the uterus.

Previously to examining whether the womb of the mother has or has not glands, and whether the infant is nourished only with the chyle or with the blood of the mother; it is first necessary to be well assured, whether it be true or not, that there exists between them a reciprocal circulation of blood; because, this fact being ascertained, will enable us more easily to know with which of those two aliments the woman furnishes the fœtus during its continuance in her womb. I shall begin therefore by examining if the blood circulates or not in each reciprocally. To effect this inquiry, I shall make use of an unfortunate event, which has opportunely happened for the absolute solution of this first problem.

A woman of thirty-five or forty years of age, forcibly detained at the general hospital, was conveyed some time since to the Hotel Dieu to lie in, being near her time, and in perfect health. This poor woman, fearing that after delivery she should be again confined, undertook to escape from the lying-in ward, which is very elevated. To execute her intention more secretly, she fastened during the night, to a window of the ward which overlooks the Rue de la Boucherie, a rope knotted at intervals, to descend more readily. But having taken her measures illy, the rope was found to be much too short, so that reaching the extremity, she was compelled to fall on the pavement below. In falling, she luxated one thigh, and broke the other, the bone protruding through the flesh, making therein a very extensive wound. In this unfortunate condition, she was carried back into the Hotel Dieu, and died in half an hour.

As soon as she was dead, one of the house surgeons opened the *belly*, which he *found filled with seven or eight pints of blood.* After removing this, he opened the uterus, in hopes of saving the infant, but perceived that it was already dead; and that *all the vessels of the woman were entirely empty of blood.* He likewise remarked that the body of the placenta, as well as the membranes, *was still united to all the internal surface of the womb;* which had prevented the blood of this woman and that of her fœtus from flowing into its cavity; as is the case when the placenta separates from the bottom of the uterus.

The union of these two parts, and so large an amount of blood discharged into the belly of this poor woman, caused me at the moment to make this reflection. The mother and child die at the same time from great loss of blood occasioned by the disunion of these parts. I have then, in order to discover if the blood circulates reciprocally from one to the other, only to examine whether the vessels of this child are void of blood, equally with those of its mother: for if they are full, it is certain that the blood of the fœtus does not pass into the vessels of the woman: but if, on the contrary, I find them empty, it is evident that a reciprocal circulation exists between them.

This idea strongly excited my curiosity, and to satisfy it, I immediately examined the little corpse. In this research I found not the slightest wound or alteration on any part of its body; and passing to the interior, all the parts appeared equally sound. Finally pursuing my examination, *I found the veins and arteries of this* fœtus *nearly, if not entirely, empty.*

Now, since the blood had not escaped into its abdomen, its breast, nor elsewhere; it is evident, that the vessels of the mother being opened by her fall, all the blood of the child had flowed with that of the mother into the cavity of the abdomen.

And, since the blood of the child could take no other route than that of the umbilical arteries and the uterine veins, in order to reach the belly of the mother; it follows consequently, that there existed during life, a reciprocal circulation between both. Nothing further then remains, but to explain the manner in which this was accomplished during the whole term of this woman's pregnancy. The facts to be stated will assuredly conduct us to this explanation.

I showed to the Royal Academy of Sciences, on the 23d of February 1706, that the uterus of a woman, who died four hours after delivery, had no glands; that its internal surface was without a membrane; that the placenta that was united with it had none on its exterior surface; that the vessels, which terminated at these two surfaces, were manifestly open, and that the substance of these two parts was fleshy and altogether spongy; and consequently enabled readily to imbibe the blood which their vessels diffused reciprocally from one to the other.

These facts being verified in more than fifty other women on whom I have performed the cæsarian section after death; and being assured that the blood of the infant of whom I have spoken was not able to escape into the belly of its mother except by taking the route of the veins of the uterus open on its interior surface to receive it, it is easy to perceive that the blood of its arteries, also open on the same part, must, during the whole period of this infant remaining in the womb of the mother, have passed into the branches of the umbilical vein, equally open on the exterior surface of the placenta, to give it a passage by the channel of this vein into the body of the infant, in order to fill the vessels which were constantly discharging into those of the mother. The four following remarks, prove still more fully this circular movement of the blood.

In the first place, we know that, after delivery, the placenta and its membranes being separated from the fundus of the womb, the blood that flows from the arteries of this part of the woman, not being able to enter the veins, spreads through its cavity and is discharged by the vagina. It cannot enter the veins, because their openings being on the internal surface, they cannot communicate with those of the arteries there terminating. When then the placenta is united to the womb, the blood which flows from its arteries ought to enter into the roots of the veins of the placenta, whilst that which flows from the umbilical arteries takes the route of the veins of the womb.

In the second place, we also know that if, during pregnancy, the placenta separates from the fundus of the womb before the woman is in labour, both mother and child perish, their vessels exhausted of blood, *how short soever the time of the discharge be**. This exhaustion could not take place if the surfaces by which the womb and the placenta are united, were covered with membranes; and if it were true that the blood of the arteries of the uterus (open on its interior surface) passed during pregnancy into its veins; and that that which is carried by the branches of the umbilical arteries to the exterior surface of the placenta, entered into the roots of the umbilical vein. Nevertheless the mother and child die, their vessels emptied by the separation of the placenta, although the woman does not fall in labour. It is evident therefore that the branches of the arteries of the uterus, which terminate on its interior surface, do not unite with the roots of the veins which there originate. It is equally the case with the vessels of the placenta. Hence, whilst the placenta remains in union with the uterus, the arteries of that ought to spread their blood in the spongy substance of the placenta, and the umbilical arteries discharge their blood into the porous substance of the womb, to be taken from thence by the veins. It is then certainly true, that between the woman and her fœtus there is a reciprocal circulation of blood. It is obviously on this account, that the surfaces by which these two parts are connected have no membranes, and that their substance is altogether spongy—hence, when separated, by pressing either of them, the blood flows from the surfaces that are not covered with membranes, but cannot escape from those parts that are covered.

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* This assertion of M. MERY must be regarded as of a general character, and as applicable to the period at which he wrote, rather than to the present, since numerous cases of flooding from the cause he mentions are recorded, in which the lives of both parent and child have been preserved, by recourse to immediate delivery;—it has however no bearing on the main point.

Thirdly,—When, on the contrary, the placenta being still united to the uterus, a woman dies in labour and her fœtus perishes at the same time by compression of the umbilical cord, then the vessels of the mother and child are equally filled with blood. The fœtal cord was free in the uterus of the female whose tragic history I have detailed; and both being dead, the veins and arteries of both were found empty; because the vessels of the mother being broken by her fall, all the blood of her infant had flowed off, together with her own, into the cavity of her abdomen. These two events, connected, evidently prove that there is between the mother and the child a reciprocal circulation of blood.

Fourthly,—If the blood of the umbilical arteries do not pass into the veins of the uterus, nor that of the arteries of the uterus into the veins of the placenta, the respiration of the mother ought to be absolutely useless in maintaining the circulation in the body of the child. If so, it is essential that the fœtus should have in itself all that is necessary to it, for the circulation of its own blood in its vessels; it might then live after the death of the mother *equally long, in the uterus*, without receiving any nourishment, as it could do, *out of that cavity*, without aliment. The reverse of all this occurs, the infant perishes as soon as the mother ceases to breathe, or if the umbilical cord of the fœtus suffers too great compression during her life. We must then acknowledge that there is between the two a reciprocal circulation of *air** and blood, and that the infant has not, *per se*, so long as it is retained in the uterus, the first principle which affords motion to its blood. The respiration of the mother is therefore the first cause of the fœtal circulation, since it perishes as soon as she ceases to breathe; and hence I conclude that an opposite opinion has every appearance of being incorrect.

Finally,—If it is true that nature acts always uniformly in the same operations, as we have reason to believe; the same should be the case in all viviparous animals, viz. that a reciprocal circulation should exist between them and the fœtus.

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* The erroneous opinion here manifest, of a reciprocal circulation of air, does not invalidate the force of the argument so ably employed by M. Mery.

ARTICLE XIII.—*Exercitatio Anatomica de Motu Cordis et Sanguinis in Animalibus, Guilielmi Harveii, Angli, Medici Regii, et Professoris Anatomiæ in Collegio Medicorum Londinensi.—Apud Harveii Opera omnia. Lond.* 1766. *4to.*

We propose to lay before our readers a few remarks and extracts from the celebrated essay of HARVEY on the Circulation; premising an expression of regret at the meagreness of this article, which however we are compelled to submit to by the crowded state of our department of original communications. Indeed it is hardly befitting so great a subject to treat of it in a short paper; for although the essay itself is a short one, and will be found on examination not to have occasioned the development of any immediate and important changes in the medical practice of the time, yet in its remote consequences it may be justly regarded as the pivot on which most of the successive improvements have turned.

A proper examination of HARVEY's work could in fact only be effected in a considerable volume. Such a volume ought to present a clear detail of the principles of medicine as they were maintained anteriorly to his era; a judicious exhibition of anatomical, physiological, and therapeutical science as they existed in the schools, hospitals, and private ministrations of eminent men during his lifetime; an analysis of the work itself; and a history of its progress, contests, and final reception and triumph, over all Europe. In this way a vast fund of interesting anecdote and useful information might be accumulated; and if it could be closed by a history of the progress of *solidism* down to the present period, it would certainly constitute a most valuable addition to the medical library: for, since the most of modern pathology is embraced in the modifications of the living blood vessels, such a work would furnish a concentrated view of the principal laws of that department of medicine. It seems scarcely necessary to say that we have neither leisure, space, nor materials for such an undertaking. Such a task could not be performed in the United States in the present condition of our medical libraries.

WILLIAM HARVEY, the discoverer of the circulation, was born at Folkstone, in Kent, England, on the 1st of April 1578. After spending six years at Cambridge, where he was admitted in 1603, he proceeded to Italy, in order to study medicine under the celebrated masters who then taught at Padua. JEROME FABRIZIO, called from his birth-place Fabricius ab Aqua-pendente, gave lectures and demonstrations in anatomy: Thomas Minadous and Julius Casserius both taught in the same school; the former, practical medicine; and the latter, surgery.

Fabricius was the successor of Fallopius, who himself had been preceded by the great Vesalius; so that Harvey expected, naturally, to enjoy the highest advantages which the then modern anatomy might bestow, in a school illustrated by such great names. Human dissections were not rare, and the demonstrations, made from the actual subject, and illustrated by frequent exhibitions of comparative anatomy, were thought to be very perfect and satisfactory. Some advances had certainly been made in the knowledge of particular structures; but the great points of the science were still handled in the Galenical manner, no man daring to differ ever so slightly from that great master, without much precaution, abjuring of all intentions to fly in the face of authority, and free confession of his paramount claim to confidence and faith. Notwithstanding all the advantages of this great school, Harvey enjoyed fewer lights than the humblest student of the present day; for at that period no lymphatic, lacteal, capillary or arterial office was even suspected, if we except what Asellius had written on the lacteals, and which seemed to have been forgotten or despised. The heart's true motion and function were unknown, and the vital functions, misunderstood *ab ovo*, were misconceived of *usque ad mala*.

It is very common, when any thing is said about the older writers, to laugh to scorn their whole system of physiology, pathology and therapeutics—and it would seem at first view, that they must have been subjected to the most deplorable embarrassments in the theory of medicine, by their ignorance of the circulation, of absorption, and the nervous functions. On the knowledge of the first of these functions are mainly dependent all the principles of modern pathology; and since we treat as impostors all those who undertake to manage the sick, being ignorant of this *abecedarium* of medicine, what respect, it is asked, can we have for a pragmatical set of old pretenders who were no better than quacks.

But this is a great mistake. Those old physicians were men of consummate skill: their histories of diseases have not been and probably cannot be improved; their prognostics excite our astonishment; and the success of their practice was often rewarded by the confidence of a whole people. The diagnostics of Galen in the case of Marcus Aurelius, and the conduct of Pare in that of the lord of Martigues, would be a subject of pride even to such men as Cooper or Broussais. The fact is they did better, by profound investigations of the tendencies, stages, and external characters of diseases, what we do worse, by our ready principles, which, anticipating personal experience and observation, seem to do away the necessity of severe labour:—their works, which contain their methods of thinking, may teach us how to learn to think:—a method, which, with all their disadvantages, led them to happy results is at least worthy of our most careful study.

Harvey learned at Padua, that the first product of digestion (the chyle) was attracted (absorbed) from the intestinal surfaces by the mesenteric veins, which, converging towards the liver, delivered it into that organ, where by virtue of a certain faculty it was converted to blood,—this conversion of chyle into blood, or hæmatosis, was effected by the influence of the ipsa epatis caro; a specific power of conversion, or facultas alteratrix, residing in every organ or system of the body, brought about these mutations,—the liver alone was the seat of sanguification and the source of all the veins of the body. Blood therefore, manufactured in the organ which gave origin to all the veins, was conveniently sent out by those veins to the parts—the arteries and heart had nothing to do in the matter of circulation. He was taught that the blood moved up and down in the veins according to the necessities of any particular case, without having any fear of the valves before its eyes, and without regard to any other of the manifest impossibilities of such an absurd ebb and flow *ad instar Euripi.*

Some supposed it to flow to the parts they knew not how; but others were satisfied that it got to the parts by virtue of an attraction exactly like that which M. Broussais and his friends suppose to operate at the present day in all cases of vascular irritation, i. e. the parts which wanted a supply had only to dilate themselves just as the thorax is dilated by the act of breathing, and the blood immediately rushed towards the vacuum, impelled by the double power of venous contraction from behind, and attraction, suction or dilatation from before it. When the blood had reached the *parts*, it was dismissed from the care of the vessels and delivered over to the *intervascular spaces*, where it became gradually converted into flesh, membrane, &c. &c. as the case might be, but never returned to the heart again ; it was only formed in proportion to the necessities of nutrition.

The whole of this doctrine is clearly explained by **Fernelius** at page 248 of Vol. I. of his works, and it strikes us as not a little remarkable that this celebrated Frenchman should have held in 1540, when he was totally ignorant of the circulation, a doctrine as to attraction of fluids by irritation, and inter-vascular spaces or parenchyma, so exactly similar to that of his celebrated countryman of the physiological school. For it will be at once perceived that he and M. B. are quite agreed on these two points of doctrine. **Galen** had settled it as a proven fact, that blood is not naturally contained in the arteries, for they always were found empty after death. The hemorrhages from arteries occasioned by wounds never convinced the profession that this idea was false ; the emptiness of those vessels could only be accounted for on the supposition of their having during life contained an intangible and imponderable fluid called vital spirits, the fruit of an union between air sucked into the lungs, changed by their organic sensibility, as above said, and conveyed to the left side of the heart by the pulmonary veins, and blood which transuded through the septa of the heart from the right to the left side. These vital spirits conveyed heat and life to the parts into which they were driven.

It is true that some modification of this doctrine had been effected by the discoveries of Servetus and Columbus, viz. that the blood of the right ventricle passed by the pulmonary artery to the pulmonary veins and so on to the left side of the heart ; but still, this glimpse of real anatomical physiology altered no body's opinion concerning the uses of the lesser circulation, the blood of which was still supposed to get to the left ventricle only for the purpose of compounding the vital spirits. Even the knowledge of the valvular structure of the veins, which had been known long before Fabricius's time, never prevailed on them to give up the old doctrine of a progressive motion of the blood from the larger trunks of veins to the smaller, in what we now know to be a retrograde and impossible direction ; this too, while the argument of the valvular structure of the heart was triumphantly used to establish the lesser or pulmonary circulation.

As we do not intend to go into an investigation of the state of the anatomical and physiological knowledge of that period, we will satisfy ourselves with repeating that **Harvey** found the science when it was destitute of any knowledge of absorption, of circulation, or of the functions of the brain.

Having returned to London, he was in 1615 elected to be a lecturer on anatomy to the College of Physicians. He began his course in 1616, and *then* unfolded his unheard of doctrines. The original manuscript of these lectures, still preserved in the archives of the College, is entitled "Prælectiones Anatomiæ Universalis, per me Guilielmum Harveium, Med. Londinens. Anatomiæ et Chirurg. Professorem, A.D. 1516, prælect. April. 16, 17, 18."

These prælectiones were the first beams of that bright day of medical knowledge which their publication shed over the nations. Having laboured earnestly on the subject, which there is reason to believe he brought to a conclusion about 1623, he sent it to press in the city of Francfort in Germany, where it appeared, not however until the year of our Lord 1628, under the title of "Exercitatio Anatomica de Motu Cordis et Circulatione Sanguinis in Animalibus."

The book is dedicated to his master, king Charles the First.

He has given a proemium or preface, in which, having demonstrated the falsity of preceding opinions concerning the motions of the heart and blood and the functions of the arteries, he proceeds to the main object of his labours in Chapter I.

> "When by the dissection of living and dead animals, and not by reference to the writings of others, I applied myself to investigate the motions and functions of the heart, I found the task so arduous, and full of difficulties, that I almost became convinced the motion of the heart could only be known to God. I could neither understand *how* the systole or diastole was effected, nor *when* or *where* the dilatation or constriction took place; because of the celerity of the motion, which in many animals occurred in the twinkling of an eye, or like a flash of lightning. Sometimes I thought I could perceive the systole, sometimes the diastole, at one while the motions were reversed, and at another utterly confused; so that my mind fluctuated. I could neither decide for myself nor receive the opinions of others, and did not wonder at Andrew Laurentius's assertion that the heart's motion resembled the flux and reflux of Euripus as described by Aristotle. At length, by means of greater diligence in the examination of many animals, and the collation of different observations, I thought I had mastered the subject, and extricated myself from this labyrinth of doubts by a discovery of the motion and use of the heart and arteries as I had desired.
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> "From this time I feared not to impart my opinions privately to my friends, and even publicly, in academic form, in my lectures on anatomy."
>
> "Some were well pleased, others not; and these latter began to confute and calumniate me, making it out a monstrous thing that I had departed from the faith and doctrines of all preceding anatomists. The former, when they saw it new,

worthy of more inquiry, and likely to be of great utility, begged to have it more fully explained to them.

"Being at length moved by the entreaty of my friends to make them participators of the fruits of my labours; and partly induced by the envy of others who, receiving my words with an evil temper, and not understanding them well, endeavoured to traduce me with the public; and, in order that every body might be enabled to judge both me and my doctrine, I was induced to commit these pages to the press."

He mentions a further inducement to their publication, viz. that his learned preceptor FABRICIUS had omitted to speak of the heart in his works.

The second chapter contains remarks on the appearances presented by the heart during the vivisection of animals. He asserts that the *living* organ is observed to exist in two different states: one of rest or inaction, the other of action or motion; which succeed each other in a regular manner.

During the state of rest it is flaccid, as we feel it in death; it is relaxed, or enervated. In its state of motion three things are observable; viz. 1. It becomes erect or rigid, and strikes against the breast with its apex so as to be felt on the outside; 2. It is contracted in all its dimensions; 3. It becomes harder, as is manifest to those who take hold of it during the motion, and assumes a whiter colour in consequence of the expression of its blood, which returning to it during the relaxed state gives its sanguineous purple hue back again.

He declares the tension of the heart, the erection of its apex, the stroke against the ribs, and the pulsation of the arteries in the *whole* body, to be dependent on its state of action, not of rest. Hence the erroneousness of the common opinion, viz. that the heart was filled with blood at the moment of its stroke against the ribs, whereas the heart is emptied and not filled by the action.

In Chapter V. he shows that the auricles both contract at the same moment, and force their blood into the ventricles, which being filled, the heart immediately erects itself, contracts the ventricles and throws the blood simultaneously into the two arteries, the pulmonary on the right and the aorta on the left. Such is the harmony or rhythm of these motions, that the four appear to be only two. After this he takes up the proofs of the lesser or pulmonary circulation which had before been announced to exist by COLUMBUS and SERVETUS; but still the function was not well understood, the nutritious office of the pulmonary artery not yet having been abandoned. HARVEY's arguments are drawn from the fact that if the septum of the heart were to be removed no obstruction would exist against the flow of blood down the aorta; from the foramen ovale and ductus arteriosus of the fœtus; from the structure of animals with a single heart, &c. "In perfectioribus nimirum et calidioribus animalibus, iisque adultis (ut in homine) sanguinem de dextro ventriculo cordis per venam arteriosam in pulmones, et inde per arteriam venosam in sinistram auriculam, et inde in ventriculum cordis sinistrum, permeare contendo: et primum posse hoc fieri; deinde ita factum esse."

In the eighth chapter he proceeds to establish his immortal doctrine of the great or aorto-cavic circulation, and evidently trembled on the verge of that precipice down which he dared to hurl the whole structure of the ancient physiology of the vascular system.

"When," says he, "I shall have spoken what it remains for me to say concerning the quantity and abundance of blood that passes through the heart, it will be so new and unheard of a proposition, that I have to fear not only evil from the envy of some men, but that I shall make all men my enemies; so much doth custom, deep rooted prejudice in favour of a doctrine once imbibed, and a venerable regard for established opinions, weigh with mankind: but be this as it may, the *die is cast*, and my hopes repose on the candour and love of truth of the learned. Truly, when I often reflected on the great quantity of blood I had observed in my experiments and investigations in living animals, and in opening the arteries, on the symmetry of the ventricles, and size of the veins and arteries which arrive at and depart from the heart; when I considered that nature who makes nothing in vain would not have given such a proportion to these vessels for nothing; when I observed carefully the structure of the valves, fibres, and other parts of the heart, and many other matters; I began to ask myself what quantity of blood is transmitted, in what time is the transmission effected, and whether the juice of our ingested aliment was capable of furnishing this quantity of blood for transmission. I reflected that by such transmission the veins would become completely exhausted and empty, while the arteries, on the other hand, would become filled and ruptured by the intrusion of such a quantity of fluid, *unless they could relieve themselves by pouring it into some other place, and finally into the veins by which it could return to the right side of the heart.* I began to reflect whether it might not have a *movement as in a circle*, and afterwards found this reflection just, and that the blood is protruded and impelled from the heart through the arteries into the whole body by the pulse (contraction) of the left ventricle of the heart (as happens in the lungs from the right to the left side through the pulmonary artery), thence by the veins to the vena cava and to the right auricle, just as occurs in the motion of the blood through the lungs."

We conceive it impossible to find any where a specimen of analysis more complete than that which HARVEY effected in studying this important subject. He did not fall upon his discovery by chance. Having examined separately all the materials, he then by a synthetic process conducted according to the best plan and marked by a perfect candour, arranged its various parts, and presented to mankind the great truths of the circulation with a clearness, justness, and perfection which has left little or nothing for posterity to do. Dr **William Hunter** seemed little disposed to wonder at and admire this great discovery, thinking it not so strange that **Harvey** effected it, as that it remained so long uneffected. But if the blessings it has conferred on mankind are inestimable, then we should hold him in the highest esteem; as he procured them and gave them to us, not by chance or by stupid good luck, but by a philosophical and laborious application of his vast intellectual powers trained and drilled to the very purpose. Truly the immortality of **Harvey**, like that of **Cadmus**, **Columbus**, and **Arkwright**, was well earned, and ought not to be subject to any questioning.

Our limits in this department being unusually narrow, we are compelled to omit the notice of the subsequent chapters of the treatise; but it seems hardly necessary to follow him closely through his arguments, which, at great length, are deduced from a multitude of facts that place the doctrine on its irrefragable basis.

Harvey supposed the extremities of the veins to be endued with the absorbing power, for he appears not to have even heard of **Asellius**'s discovery of the lacteal vessels, which had been published in 1622.

It may be easily imagined that so signal a discovery would not fail to occasion much discussion in a profession so much given to disputing as ours—and we are sorry that the common complaint of "too many Medical Journals" could not have been made at that day, or we should have had reviews, cases and quarterly summaries of *circulating* matter. Poor as the press was in that day, a considerable number of works were published, either modifying, advocating or rejecting the doctrine. Harvey himself did not escape the fate he seemed to fear; for if not all, yet most men became his enemies, and he lost his reputation and practice for what ought to have admitted him into the calendar at least, if not to some higher honour. He was pointed at as the *circulating man.* Nevertheless as he lived to an advanced age, he outlived all opposition, and found his doctrine received all over Europe. The immediate results of the discovery, as affecting the practice of physic, are not very evident. It is probable that the chemical doctrines of that day had much to do in retarding the beneficial tendencies of a clear view of the circulation. Salts, sulphur, and mercury, fermentations, concoctions, &c. &c. stood mightily in the way of a dispassionate contemplation of the influence of vascular action on the vital phenomena. It could not be, however, that the same obscure notions should exist in regard to the uses and objects of blood-letting, which, divested of the absurdities of the galenical physiology, could now be practised on good principles.

After Pecquet, in 1645, had shown the course of the chyle through the lactiferous tubes of Aselli into the thoracic duct, and thence into the stream of the circulation; and when Bartholine and Rudbeck had described, in 1651, the lymphatics as far as they could discover them, nothing seemed to hinder the progress of the science, and it made daily acquisitions through the labours of Glisson, Boerhaave, Whytt, and Haller, until its vast form having been at last blocked out by the rough hands of John Hunter, received the polish and elegance of a finished work by the masterly skill of Bichat.

After all, we owe to Harvey whatever we possess of true anatomy, physiology, or therapeutics; for neither one nor the other of these departments could have ever existed in their present perfected state, but for the happy direction and philosophic employment of his admirable genius and industry.

We take leave to repeat our regrets that some able pen has not been employed to pourtray all the relations and dependencies of our science on the labours of Harvey. A strict and just comparison of the modes of practice, and its results, before and after the publication of his work; an examination of the causes which retarded in some degree the development of its influence; or even a modernized paraphrase of the doctrine it inculcates, would have a salutary tendency in diffusing a better taste, and at the same time expose a most excellent body of doctrine on all that portion of pathology which is conversant with the morbid modifications of our vascular system.

Bibliographical Notices.

ARTICLE XIV.—*Elements of Chemistry, including the Recent Discoveries and Doctrines of the Science. By* EDWARD TURNER, M.D. F.R.S.E. *Fellow of the Royal College of Physicians, and Lecturer on Chemistry, Edinburgh, &c. Edinburgh,* 1827. *8vo, pp.* 723.

DR TURNER's work is divided into four parts: I. Imponderable substances. II. Inorganic chemistry. III. Organic chemistry. IV. Analytical chemistry. The second part, on inorganic chemistry, is treated of under four divisions. The first division comprises affinity, the laws of combination, the atomic theory and theory of volumes founded thereon, and the description of the simple non-metallic bodies, together with their compounds with oxygen, chlorine, iodine and fluorine. The second division treats of the remaining combinations between the non-metallic bodies, under the title of "compounds of the simple non-metallic acidifiable combustibles with each other." The third division is occupied with the metals, and the fourth, with salts.

This arrangement would seem to be imperfect. After giving Part I. for the imponderable substances, we naturally look in Part II. for the correlative title of ponderable substances, which admit very readily of being considered under the two heads of simple ponderable substances and their inorganic combinations, and organic compounds, corresponding to Dr TURNER's parts of "inorganic chemistry" and "organic chemistry." But waiving this objection, it was to be expected that some definite title would have been given to the first division of inorganic chemistry, as is done in respect to the other three divisions. This defect in arrangement is the more prominent, inasmuch as the title of the second division, by its restricted tenour, clearly implies that certain non-metallic combinations, selected on some principle of classification, have been already treated of in the first division. Besides, the subjects of affinity, laws of combination, atomic theory, do not harmonize well with descriptions of the non-metallic bodies, as treated of in successive sections of the same division of a scientific arrangement. Our objections would have been removed, if the author had treated "inorganic chemistry" under five heads, which might have been called, 1. "Philosophy of chemical action," embracing all that is said under the titles of affinity, laws of combination, atomic theory, theory of volumes, &c. 2. Simple non-metallic bodies, and their combinations with oxygen, chlorine, iodine and fluorine. 3. Remaining combinations of non-metallic bodies with one another. 4. Metals. 5. Salts.

Dr TURNER's arrangement here has some points of resemblance to that of Dr

HENRY. The difference consists in this, that while Dr TURNER treats of the whole of the non-metallic bodies, and their combinations with oxygen, chlorine, iodine and fluorine in one class, Dr HENRY treats of the same bodies and compounds, under two divisions; first, oxygen, chlorine, iodine and fluorine, denominated electro-negative bodies, and their combinations with each other, and secondly the remaining non-metallic bodies, and their compounds with the electro-negative bodies. Precisely the same bodies and compounds are treated of in Dr TURNER's first division, and in Dr HENRY's first and second divisions; but Dr TURNER's arrangement has the advantage of enabling a writer to treat of the non-metallic bodies in the most eligible order for teaching, without reference to an artificial arrangement, founded on the electrical relations of the bodies themselves. Thus Dr HENRY, after treating of oxygen, is forced to take up in immediate succession, chlorine, iodine and fluorine; while Dr TURNER is enabled to adopt the better teaching order, of oxygen, hydrogen, nitrogen, &c.

We hold it that a strictly scientific arrangement, and a teaching arrangement are quite different things, and that a treatise on any subject of science for a learner must be arranged in a different manner from one intended for a proficient. In a science like chemistry, in which, from the mutual dependence of facts, the chain of knowledge may be aptly compared to a circle without beginning or end, we have no other alternative than to commence with those substances, which possess the most diversified relations with other substances, and the knowledge of which may be most available in our progress. Now these advantages are secured in chemistry by beginning with oxygen, hydrogen and nitrogen, whereby the learner is made acquainted in the outset with water and the atmosphere. A knowledge of the mineral acids, and the alkalies is the next most important acquisition in the beginning; and the elementary bodies which they contain should be brought forward as early as possible, consistently with a proper regard to some arrangement as founded on the analogy of the bodies in properties. Nitric acid comes to be treated of early in Dr TURNER's arrangement by reason of the identity of its constituents with those of atmospheric air. Sulphuric acid, as a chemical agent, is even more important than the nitric; and though it might be difficult to give it an earlier notice than nitric acid, still we think it ought not to be postponed one moment longer than the necessity of the case imposes. Accordingly we should advocate the consideration of sulphur immediately after nitrogen, and chlorine next, with the view of adding muriatic acid to the stock of known compounds. After this, the remaining non-metallic bodies might be treated of in the following order:—Carbon, phosphorus, iodine, boron, selenium, fluorine, without regard to their analogies, but in the order of their relative importance.

Without disregarding entirely the object of having a class of metals, it is impossible to introduce to the learner a knowledge of the fixed alkalies and their radicals, at as early a period as their importance would render desirable. Hence it is, that in all our chemical systems we find potassa and potassium mentioned incidentally under nitric acid, and soda and sodium under muriatic acid,—anticipations rendered necessary in describing the processes for obtaining those acids.

These then are a few of the many difficulties which beset the subject of chemical arrangement; and that these difficulties have not been removed ought not to excite surprise, in a science, the objects of which are so multitudinous, and which is undergoing almost daily changes, by the contribution of new facts and observations.

After completing the account of the imponderable substances, Dr TURNER goes on to consider the subject of affinity. After this he treats of the laws of combination, the atomic theory, and the theory of volumes. The author deserves much praise for the able manner in which he has handled the latter subjects; and after reading the section devoted to them with attention, we can truly say that we have seen them discussed no where else in so successful a manner.

After this general expression of our satisfaction with this part of the author's work, we may be permitted to make some comments; and if we point out some slight defects in his reasonings and doctrine, we hope not to be charged with the desire of condemning in detail, what we commend as a whole.

At page 120—121, the author after having illustrated the law of multiple proportions by examples, in which it is shown that the different proportions in which one body combines with another have some simple relation to each other, as 1 to 2, or if there be more than two proportions, as 1, 2, 3, or 1, 2, 4, &c., he goes on to announce what he calls the general law in the following terms:—"When two substances, A and B, unite chemically, the quantities of the two bodies must either be equal, or in the ratio of multiples or sub-multiples of each other."

The law of multiples, as here given, is, to say the least, every obscurely expressed. It would seem to imply some peculiar relation in the quantity of one body to the quantity of another body with which it may combine, instead of expressing the relation of the different quantities in which the same body combines with another body. What is meant by the expression that "the quantities of the two bodies must either be equal," &c., is not easy to imagine, unless it is intended to convey the idea that the bodies must unite either in the primary proportion, or in some multiple or sub-multiple of the numbers, denoting such proportion.

At page 138, Dr TURNER says, "the theory of volumes has very considerable analogy to Mr DALTON's law of multiple proportions. The former is indeed, to a certain extent, a consequence of the latter; for if one body unites with another in several proportions, the quantities of the variable ingredient will stand in the same relation to one another, when expressed by volume, as they do by weight. But there is one remarkable difference. The weights of the two elements of a compound have no apparent dependence on one another. Thus, 6 carbon and 8 oxygen form carbonic oxide; 8 oxygen and 14 nitrogen form nitrous oxide; 8 is no multiple by any whole number of 6, nor 14 of 8. But the elements of a compound are always united by volume in the ratio of 1 to 1, 1 to 2, 1 to 3, and so on."

In the above quotation there are some truths, expressed in a somewhat loose manner. In the first sentence, the theory of volumes, which certainly embraces, not only the simple relation in which *different* gases combine in bulk, but also the simple relation of the successive proportions in which the *same* gas combines with some other gas, is compared with one branch only of the theory of combining weights, namely multiple proportions. It would have been more correct to say that *multiple* proportions by volume, using this expression in its ordinary acceptation as relating to different portions of the same body, follow as a necessary consequence from *multiple* proportions by weight, and for the reason which is so clearly expressed in the second sentence of the quotation. We may even go farther, and say, that *definite* proportions by volume follow as a corollary from *definite* proportions by weight; but how far the ratio of the numbers may be simple which express these definite proportions in volumes, can be determined only by observation, and must depend upon the specific gravity of the different gases; that is, on the relation of the spaces which combining quantities of the gases by weight may happen to occupy. It is indeed true that the relation of these spaces, or what we may call the combining volumes of the gases, is exceedingly simple; but this by no means proves any remarkable *difference* between the *theory* of volumes and Mr Dalton's law of *multiple* proportions. It indeed shows that there is a difference between *combining volumes* and *combining weights*, in the simplicity of the ratios of the numbers which express them; for while combining weights are expressed by numbers which are not even multiples of each other, combining volumes are as 1 to 1, 1 to 2, 1 to 3, &c.

Though the numbers denoting combining weights are not multiples by a whole number of each other, they possess the curious property of being multiples by a whole number of the combining weight of hydrogen, which is that element which is found to combine in the smallest proportion. This observation was first made by Dr Prout, who proved it to hold good in a number of instances. It was afterwards extended, and found to apply to a great many substances, by Dr Thomson in his "First Principles of Chemistry," a work which we reviewed in our first Volume.

It is a remarkable fact that the chemists of the European continent do not seem willing to admit the above law of Dr Prout; at least they are not found to adopt it in practice. After some study we have convinced ourselves that it is well founded. It is indeed true, that this law may be alleged to be altogether a fallacy, since in the higher atomic numbers on the hydrogen unit, the greatest deviation from an even multiple, (five tenths) will form but a small proportion of the entire atomic number, and may be considered within the range of the unavoidable errors of experiment. For this reason the higher atomic numbers cannot be appealed to with confidence in support of Dr Prout's law. But the same objection does not apply to the smaller atomic numbers; and it is on that account that we have formed the following table, which embraces all those simple bodies, the atomic weights of which are under 21; the atomic weight being given, in the first column, according to the experimental numbers adopted by Berzelius and Thenard, and in the second, according to the latest determination of the British chemists, who generally admit Dr Prout's law. The numbers are brought to the same standard, on the assumption that the atomic weight of oxygen is 8.

	Berzelius and Thenard.	*British Chemists.*
Hydrogen	.994	1.
Carbon	6.12	6.
Boron	8.60	8.
Silicon	7.40	8.
Aluminum	9.12	10.
Lithium	10.22	10.
Phosphorus	12.55	12.
Magnesium	11.77	12.
Nitrogen	14.16	14.
Sulphur	16.09	16.
Glucinum	17.66	18.
Calucim	20.48	20.

The nearest coincidences in the above table with Dr Prout's law, occur in relation to hydrogen, carbon, lithium, magnesium, nitrogen, and sulphur; while the greatest discrepancies relate to boron, silicon, aluminum, phosphorus, glucinum, and calcium. If it be asked where would be the nearest coincidences with this law, supposing it founded in fact, the answer is, they would obtain in regard to those substances which are most important, and the combinations of which have been most carefully studied. Now the substances which answer to this description in the above table are hydrogen, carbon, nitrogen, and sulphur, and here we have the nearest coincidences with Dr Prout's law. The greatest discrepancies are exhibited in the cases of boron, silicon, aluminum, and phosphorus, and these are among those chemical substances which have caused to chemists the greatest perplexity in determining their atomic weights. Hence it is fair to infer that the experimental numbers for these are more likely to be inaccurate, than those for the other substances included in the table.

If Dr Prout's law be not well founded, how are we to explain the fact that hydrogen, carbon, oxygen, nitrogen, and sulphur combine so nearly in the ratios of the numbers 1, 6, 8, 14 and 16? If the combining proportions were not regulated by some law of this kind, would we not have, for the four last named substances, atomic numbers which would not contain the atomic number for hydrogen an even number of times without a remainder? We ask then why this close approximation to whole numbers, if it depend not on a simple relation between the weights of the atoms amongst themselves?

We have been drawn into a digression on this abstruse and curious subject, under the influence of a sentiment, that Dr Turner has not dwelt sufficiently on Dr Prout's law; and because we are convinced that the chemists of continental Europe have not paid that attention to it which its importance merits. That part of the theory of volumes of Gay-Lussac which alleges that different gases combine in bulks which have a simple relation to one another, may be considered as the counterpart of Dr Prout's law; the relation of volumes being substituted for that of weights. Oxygen is the least combining volume, and if its combining volume be called 1, that of all the other simple gases will be 2, of ammonia 4, &c. The numerical expressions for volumes are indeed few, and in that respect differ from the numerical expressions for combining weights; but the principle in both is the same, namely that all the numbers in either series are even multiples of the lowest combining quantity belonging to each.

If the combining volumes of Gay-Lussac were to be determined on such rigid experimental principles, as seem to be acted on by the French and Swedish chemists in determining combining weights, we doubt very much whether it could be asserted that oxygen combines with hydrogen in bulk, in the ratio of 1 to 2. Experimental determination can do no more than furnish an approximation to the truth; and in the strict sense, it may be said to be impossible, when analysing water, to obtain the oxygen and hydrogen in volume in the precise proportion of 1 to 2. But the constituents may be obtained very nearly in those proportions; and knowing, as we do, the simplicity of nature's laws, and that gaseous analysis generally gives close approximations to simple ratios, we are satisfied to assume those proportions, which our experimental results so nearly approach. Now this is all very proper; but we would ask, why is not the same course adopted in determining combining weights. We see no difference in the two cases, except that the principle must be applied with more caution in the case of weights.

We have said more than we first intended on the theory of chemical proportions, and, therefore, shall be under the necessity of being more brief in the remaining remarks which we may have to offer.

Under the compounds of hydrogen and carbon, Dr TURNER gives the composition of olefiant gas to be two atoms of carbon + two atoms of hydrogen. We cannot conceive why the atoms must be duplicated in this case. It is true that we may say that this gas consists, in volume, of two volumes of vapour of carbon + two volumes of hydrogen, condensed into one volume; and that turning these volumes into their equivalent weights, we shall have two atoms of carbon + two atoms of hydrogen. But we would say that its composition in volume is one volume of vapour of carbon + one volume of hydrogen, condensed into half a volume; and these volumes converted into weights, would give us a single atom of each ingredient of the gas. It is true that the condensation of the elements of this gas is anomalous, and it is this circumstance, probably, which induces Dr TURNER to assimilate it to the several curious vapours recently discovered, which are identical with olefiant gas in the proportions of their ingredients, but still more condensed; and the difference between which is supposed to depend by the author, on a greater or fewer number of iterations of the same atoms to form one compound atom. But admitting the probability of this conjecture, as applied to the newly discovered vapours, it seems premature to place olefiant gas in the same class with them, and to assert that the compound of a *single atom* of carbon with a *single atom* of hydrogen is yet to be discovered.

Dr TURNER's book has the merit of being well written, a merit which belongs to few of our chemical treatises. The different subjects are treated of with neatness and brevity, and the author has evinced much judgment in the selection of the facts and statements most appropriate for a compendious treatise. Throughout, much to our satisfaction, the hydrogen unit for proportional numbers is employed, and we do not despair of seeing the day, when this unit will be universally adopted. As a specimen of the author's style, we shall make one extract, which will enable our medical readers to judge on this point for themselves, and at the same time present them with a good abstract of the latest chemical researches on the colouring matter of the blood.

"The crassamentum or clot of the blood consists of two parts, the fibrin and colouring principle. The latter resides in distinct particles, which, according to PREVOST and DUMAS, are elliptical in birds and cold-blooded animals, and assume the globular form in the mammiferous animals. These globules are insoluble in serum; but their colour is dissolved by pure water, acids, alkalies, and alcohol. Much uncertainty prevails among chemists relative to the cause of the colour of the red globules. As soon as the blood was known to contain iron, the peroxide of which has a red tint, the colour of the red globules was ascribed to the presence of that metal, and some chemists supposed it to be in the form of the sub-phosphate of iron. This opinion was adopted by FOURCROY and VAUQUELIN, who even affirmed that the phosphate of iron may be dissolved in serum by means of an alkali, and that the colour of the solution is exactly similar to that of the blood.

"This subject was investigated in 1806 by BERZELIUS, who showed that the sub-phosphate of iron cannot be dissolved in serum, in the way supposed by Fourcroy and Vauquelin, except in very minute quantity; and that this salt, even when rendered soluble by phosphoric acid, communicates a tint quite different from that of the red globules. On comparing together the composition of the three principal ingredients of the blood, viz. fibrin, albumen, and colouring matter, he found that the ashes of the last always yielded oxide of iron in the proportion of 1-200th of the original mass, while the oxide was entirely wanting in the two former. From this it was a probable inference that iron is somehow or other concerned in the production of the red colour; but the experiments of Berzelius did not make known the state in which the metal exists in the blood. He could not detect its presence by any of the liquid tests. (Med. Chir. Trans. Vol. III. p. 213.)

" In a series of experiments published in 1812, (Philos. Trans.) Mr Brande obtained results quite contrary to those of Berzelius. He detected iron in the ashes of the serum and fibrin as well as in those of the red globules; and in each it was present in such minute quantity, that no effect as a colouring agent could be expected from it. Mr Brande supposed that the tint of the red globules is produced by a peculiar animal colouring principle, capable, like other substances of a similar nature, of combining with metallic oxides. He succeeded in obtaining a compound of the colouring matter of the blood with the oxide of tin; but its best precipitants are the nitrate of mercury and corrosive sublimate. Woollen cloths impregnated with either of these compounds, and on their being dipped into an aqueous solution of the colouring matter, acquired a permanent red dye, unchangeable by washing with soap.

" The conclusions of Brande, relative to the presence of iron in the albumen and fibrin of the blood, received additional support from the researches of Vauquelin; (An. de Ch. et de Ph. Vol. I.) but the question has been finally decided by Dr Engelhart, a young German chemist of great promise, who gained the prize offered in the year 1825 by the Medical Faculty of Gottingen for the best essay on the nature of the colouring matter of the blood. (Edinb. Med. and Surg. Journal for January 1827.) He demonstrated that the fibrin and albumen of the blood, when carefully separated from colouring particles, do not contain a trace of iron; and, on the contrary, he procured iron from the red globules by incineration. But he has likewise succeeded in proving the existence of iron in the colouring matter of the blood by the liquid tests; for, on transmitting a current of chlorine gas through a solution of the red globules, the colour entirely disappeared, white flocks were thrown down, and a transparent solution remained, in which the peroxide of iron was discovered by all the usual re-agents. The results obtained by Dr Engelhart relative to the quantity of the iron, correspond with those of Berzelius. These facts have been since confirmed by M. Rose, who has accounted in a satisfactory manner for the failure of former chemists in detecting iron in the blood while in a fluid state. He finds that the oxide of iron cannot be precipitated by the alkalies, hydrosulphuret of ammonia, or infusion of galls, if it is dissolved in a solution which contains albumen or other soluble organic principles.

"From the presence of iron in the red globules, and its total absence in the other principles of the blood, it is probable that this metal, though its quantity does not exceed one-half per cent., is essential to the production of the red colour. The experiments of Dr Engelhart, however, have not determined the manner in which it acts, nor in what state it exists in the blood, though it is most probably in the form of an oxide. It is a singular coincidence that the sulpho-cyanic acid, which forms with the peroxide of iron a colour exactly like that of venous blood, has been detected in the saliva. The existence of this acid in the blood itself is, therefore, a circumstance by no means improbable."

The above extract furnishes a fair specimen of the general style of the author; but we are by no means to infer that he possesses the inferior merit only of a good compiler of the labours of others. On the contrary, Dr Turner, although a young chemist, has already made himself favourably known by several detached experimental memoirs. The estimation in which he is held by the British public as a promising chemist may be inferred from the fact, that he has been lately called from Edinburgh to fill the important professorship of chemistry in the London University.

In perusing Dr Turner's volume, we have discovered the following errors, which we mention for the benefit of those students who may be reading the work.

Page 22. Speaking of the expansion of water:—" as 4.7 to 15," for, 14.7 to 15.

Page 83. " Thus, if one pound of mercury at 180°" for 185°.

Page 116. " *Silver* retains its lustre," for *gold*, &c.

Page 135. The contraction of volume in the formation of sulphurous acid is stated to be " *to a third*," instead of *to a half*.

Page 169. " That some of the *peroxides*," &c. for, that some of the *protoxides*.

Page 190. " When an acid is added to the *hyponitrate* of potash," for *hyponitrite* of potash.

Page 220. " Gay-Lussac found that [sulphuric acid] is resolved into two measures of *sulphuric* acid and one of oxygen," for, *sulphurous* acid and one of oxygen.

Page 225. " According to Dulong, [hydrate of phosphoric acid] is composed of 36 or three atoms of phosphoric acid," for, 84 or three atoms of phosphoric acid.

Page 245. " The protoxide of *hydrogen*" is printed for protoxide of *chlorine*.

Pages 249—250. Speaking of perchloride of carbon, it is said, " alkalies do not act upon it; nor is it changed by the stronger acids, such as the muriatic, nitric, or sulphuric acids, even with the aid of heat; charcoal is separated, and muriatic acid gas is evolved." There is evidently something omitted before the word charcoal, necessary to make the sense complete. The context seems to require these words,—" when transmitted along with hydrogen through a red-hot tube," charcoal, &c.

Page 282,

" Hydrogen . . 4.236 . . 2 or *one atom*,"

for 2 or *two atoms*.

Page 301. " Dr Thomson states, that water and *sulphurous* acid result," for *phosphorous* acid.

Page 304. " The instant [sulphuretted hydrogen] comes in contact with the cyanuret [of mercury], double decomposition ensues, and hydrocyanic acid and the black sulphuret of mercury are generated." This sentence contains a mistake, if by black sulphuret Dr T. means the proto-sulphuret. The cyanuret of mercury, being a bicyanuret, there is obviously generated a bisulphuret of mercury, or cinnabar. Further on Dr T. appears to be aware of this mistake; for at page 438, he says, "I apprehend the black precipitate, formed by the action of sulphuretted hydrogen on the cyanuret of mercury is likewise a bisulphuret."

Page 318. "*Bicarburet of sulphur*" is printed for *bisulphuret of carbon.*

Page 353. The process, which is designated as the best for obtaining *lithia*, results in obtaining the *sulphate of lithia.* In a treatise, intended for learners, it ought not to be taken for granted, that they will know how to obtain pure lithia from the sulphate.

Page 358. "108 parts or *one* atom of water" for 108 parts, or *twelve* atoms of water.

Page 362. "The composition of chloride of *calcium*" for chloride of *lime.*

Page 364. Magnesium is said to be "converted by *mixing* with oxygen into magnesia."—Same page, magnesia is stated to be "a white *fusible* powder."

Page 378. "By conducting *fluoric* acid gas," &c. for *fluo-silicic* acid gas.

Page 418. "The peroxide of uranium is decomposed by a strong heat, and converted into the *peroxide,*" for *protoxide.*

Page 425. Protoxide of copper is said to combine with sulphuric acid. We never heard of the salt which in this case would be formed, our impression being that all the salts of copper were per-salts, except one, the protomuriate. In the same page, ammonia is said to throw "down a greenish-blue sub-sulphate," without any thing being said in the context about the sulphate of copper.

Page 428, table of the oxides of lead. *Protoxide* is printed for *peroxide*, and 124 for 120.

Page 483. Some of the salts of the peroxide of mercury "are converted by water into insoluble yellow sub-salts and soluble colourless *per-salts*" for *super-salts.*

Page 485. "The chloride of mercury [meaning corrosive sublimate] is probably converted, at the moment of being dissolved, into a muriate of the *protoxide,*" for *peroxide.*

Page 485. "The *carbonate* of lime is separated by filtration," for *phosphate* of lime, &c.

Page 490. "The yellow chromate which consists," &c. for the yellow chromate of *lead.*

Page 505. "The hydro-sulphate of baryta and strontia, prepared by dissolving the sulphurets of barium and *strontian* in water," for *strontium.* Same page. "If the alkali is completely saturated with the gas, the resulting compound is a *bisulphate*" for *bi-hydrosulphate.*

Page 521. The water in acetate of soda is stated to be "73 parts or six atoms," instead of 54 parts, &c.

Page 523. The crystals of the binacetate of copper are stated to "consist of *one* atom of acid and *two* atoms of the peroxide of copper," instead of *two* atoms of acid and *one* atom of the peroxide.

Page 527. Oxalate of lime is stated to consist "of 36, or one atom of the acid, and 23 parts or one atom of lime," instead of 28 parts or one atom of lime.

Page 531. "Mr Phillips recommends that 100 parts of metallic antimony in fine powder should be boiled to dryness in an iron vessel, and that the residual subsulphate," &c. After "vessel" the words, *with* 200 *parts of sulphuric acid,* are wanting, to make the sense complete.

Page 564. "In cider, *sherry*, ale and porter," for, In cider, *perry*, ale and porter.

Page 567. "According to this chemist, *sulphuric* acid and the oil of wine are both composed of sulphuric acid and carburet of hydrogen," for, according to this chemist, *sulpho-vinic* acid, &c.

Page 594. "When the gum of the olive *oil*," for, When the gum of the olive *tree*.

Page 601. "The information contained in chemical works relative to the substances susceptible of the *vinous* fermentation," &c. for, *acetous* fermentation.

Page 616. "When fibrin is mixed with its own weight of concentrated sulphuric acid, a perfect solution ensues, without change of colour, or disengagement of *sulphuric* acid," for disengagement of *sulphurous* acid.

Page 687. "*Mode of analyzing an earthy mineral containing silica, alumina, manganese, lime, and magnesia.*" The directions given under this head contemplate the presence of iron also.

Page 690. "Hard water is characterized by decomposing soap, the lime of the former yielding an insoluble compound with the *acid* of the latter," for *oil* of the latter.

Page 704 *et seq.* The table of proportional numbers is carelessly printed. The atoms of oxygen for example, are evidently intended to be denoted by the initial of that element, followed by the atomic weight; thus, o. 8, o. 16, o. 24, &c.; but by a singular mistake of the printer, the whole is invariably printed, as if it were a decimal; thus, as applied to the above instances, 0.8, 0.16, 0.24. Sometimes the *number* of atoms follows the initial; at other times, the *aggregate weight* of the number of atoms intended to be expressed. This want of uniformity in plan is apt to mislead the student.

Besides the above, we have detected a few additional errors of less importance, which we have not room to indicate. Upon the whole, we think it not going too far to assert, that, whatever may be the other merits of Dr TURNER's book, it contains many more errors than can be properly ascribed to the intrinsic difficulties of correcting the press.

Mr John Grigg of this city has lately published an American edition of Dr TURNER's work. We are glad to see this, as we consider the work to be valuable on the whole, and as having the advantage of being neither too copious in the detail, nor too compendious; but we regret very much that the reading public in this country should furnish a demand for books, executed in the form and manner of this reprint, with all its disadvantages of duodecimo size, small type, close lines, and inferior paper. It is perceived that we lay the whole blame upon the public; for we assume it as not to be doubted, that if they desired a better commodity, and were willing to pay the price, it would be furnished with all willingness by the bookseller.

It is to be regretted that the Edinburgh copy had not been revised before reprinting it; as then the errors which we have noticed above might have been avoided. As it is, they have been perpetuated, with one or two exceptions, and indeed, added to by the unavoidable inaccuracies of all new impressions. Though, as we have said before, we cannot approve of the cheap style in which the American edition is got up, yet it is but justice to say, that the type, though small, is good, and the impression, clear and distinct.

We conclude by recommending Dr TURNER's treatise to the attention of our chemical and medical readers; and by those who wish to possess the work, and who may not find it convenient to procure the more costly Edinburgh copy, the American edition will be found a convenient and cheap substitute.

Quarterly Summary

OF

Medical and Surgical Intelligence.

I. ANATOMY.

1. *Dr* **Weber** *on the Skin.*—Dr **Weber** is professor of anatomy at Leipzig. In opposition to the opinion of Dr **Eichorn** whose observations were cited in a former number of this journal, Dr W. asserts that the sebaceous follicles of the skin are organs distinct from the *bulbs of the hair*, and that they exist over the whole surface, excepting the palms of the hands and soles of the feet. The bulbs of the large hair (gros poils) are situated very deeply in the derm, and sometimes penetrate even into the subcutaneous adipose tissue: the sebaceous follicles on the contrary, are nearer to the cutaneous surface, and are never found extending to the adipose structure. Their size also, says he, is too large, to permit them to be confounded with the bulbs of hair, which are much smaller. In new born children, sebaceous follicles may be discovered on all parts of the skin with the two exceptions already named. The skin of the scrotum shews them very much developed; each of these follicles is composed of four or five compartments or cells agglomerated together; their transverse diameter exceeds their depth. The greatest diameter observed by the author was a quarter of a line.

Dr **Weber** also examined the hair of the body in order to ascertain whether it is hollow, but he found no canal, or any cellular structure within it. Neither is this hair cylindrical; it is rather elliptical or oval like the negro hair, in which the flattened form seems necessary to admit of the curl or twist.

2. *Singular Case of Mutism.*—A young man, about twenty years of age, fell two years since from a height of ten or twelve feet; he lighted on his heels, and by the *contre-coup* experienced a concussion of the brain, was stunned for half an hour, and ceased to speak freely. After two years he was brought to the Hotel-Dieu, as affected with hemiplegia. M. **Dupuytren** received him, and soon ascertained that he was affected neither with hemiplegia nor paraplegia. The young man could walk and use his arms, but on attempting to answer a question he hesitated, manifestly from consciousness of the difficulties he had to encounter. He made vain efforts and could scarcely get out a few words badly articulated. There was nevertheless no appearance of idiotism. He appeared to understand the words addressed to him.

Is the disease local? asks M. D. Does it depend on some particular affection, (*vice propre*) on a paralysis of the tongue? or does it arise from an affection of the brain at the origin of the nerves that move this organ? Is there paralysis of both sense and motion, or does he still retain the power of distinguishing savours? The solution of this last question would not be difficult, and M. D. supposed that the solution of this might involve that of the former.

If the patient still preserved the perception of savours, if the nerves of motion only are injured, the cause of the affection might be found in the part of the brain that gives origin to them: if, on the contrary, sensation is gone, if taste has disappeared, then, the nerves of taste being different from those of motion in their origin, it will be difficult to conceive of a simultaneous affection of two parts so distant from each other. In that case we must seek for the cause and seat of the disease in the nerves of the tongue, in the organ itself.

M. Dupuytren now caused the young man to be taken to the amphitheatre: he was pale, lean, of a common height; his countenance a little embarrassed; he appeared to be at once bashful and pensive. He was asked the nature of his profession. *Mine* - - - replied he with difficulty; at length he was enabled to get out *Mineur*. His age? *Vingt* - - - *t* - - - *t* - - *d* - - *eux ans*. His name? *Jacques Col - in* - - *Col* - - *ar* - - *Col* - - *ard*. Have you any brothers and sisters? *Oui*, said he. How many brothers? *Un*. How many sisters? *Trois*. Are they married? *Oui*. What is the business of your brother? *Peintre*. This word was pretty well pronounced. What is the business of your eldest sister's husband? *V* - - *V* - - *Ver* - - -, said he. Is it *Vitrier* you wish to say? asked M. Dupuytren. He makes a negative gesture. Does he make bottles? Same sign—*V* - *V* - *Ver*—repeats the patient. Do you understand well what I say to you? *Oui*. Strike on the table. He struck. Lift up your foot. He lifted it up. Put it to the ground. Turn your head to the right—to the left. Look up at the sky—down to the ground. Get up. Go to the right—to the left—straight forwards. Come back. Sit down. Put on your cap. All these orders were obeyed immediately, and all the motions executed with promptness and precision.

"The *difficulty* seems to be in the tongue only," said M. D. "*Non*," replied the patient, thrusting out the tongue to shew it was free and sound. "The *difficulty* is in the tongue, the muscles of the larynx are doubtless not connected with it; it is the organ of speech, the modificator of sounds, that is affected: the brain appears to be free also."

"Let us continue the examination; pronounce the words that I am going to say to you—*Sa, se, si, so, su*." He pronounced them without much difficulty, except the *so* and *su*, which his tongue refused to repeat: he could only say *o*—*u*. "Say *mon père*." He answers "*pé* - - - ." "*Ma mère*." He answers "*mé*." "Do you retain the sense of taste?" said M. Dupuytren. "*Oui*." "Do you understand my question? When you get soup does it taste as it used to? Does some food taste good, and some bad? Can you taste salt and pepper?" "*Oui*." "We will ascertain that point. We will make the trial at the next examination" (seance): and the young man, with more haste than politeness, began to leave the place, without saying any thing. "Stop, my friend, is this the way you intend to leave these gentlemen? Pull off your cap, and bid them adieu." He takes off his cap. "Well, say adieu." "*Ad* - - - *d* - - - *eu*," said he, and left the room.

April 9. The trials intended for Monday were deferred till this day (Wednesday). The young man appeared to dread them, and submitted to them reluctantly. Three substances were to be employed—salt, sugar, and pepper. In the first place, M. D. wished to ascertain whether he could pronounce these words sufficiently well to obviate the danger of attributing to a defect of taste what might depend only on a fault of speech. He said *sel* very well; the words *sucre* and *poivre* were pronounced imperfectly. Some salt was then placed on his tongue, and he recognised this substance: but he afterwards mistook sugar for salt, and called the pepper *rum*. Some one remarked that he was from a district where little wine, but a great deal of brandy was drunk, which made M. D. think he had confounded the taste of rum with the biting quality of pepper. He appears nevertheless to know the taste of wine, which he has asked for several times since he has been in the Hotel-Dieu. He was next made to drink some water, which he swallowed and recognized without any difficulty. Recollecting the observations frequently made after concussion of the brain, chronic affections of the organ, and apoplexies, M. D. is tempted to modify, or rather, completely to change the opinion he had formed in the onset. After affections of the brain we in fact frequently observe both loss of memory and forgetfulness of things and words, of proper names, of substantives or adjectives; some patients lose the faculty of comparing objects and forming a judgment of them. We have seen an old woman who after an attack of apoplexy, followed it is true by hemiplegia, although she retained the free use of her intellectual faculties, and understood all the questions that were addressed to her, yet could only answer by these words frequently repeated—*Saint Antoine, Saint Antoine.* After all, in these different patients the intellectual faculties appear to be, in general, sound; there appears to be no other lesion of the senses, and we might according to M. DUPUYTREN attribute the affection to a general lesion of the brain, influencing secondarily the functions of this or that sense, rather than to a direct and local affection of two different points of one organ.—*Nouvelle Bib. Med. Avril.*

II. PHYSIOLOGY.

3. *Rumination.*—M. RICHE of Maubeuge, writes to M. BROUSSAIS, that a young man aged seventeen years and a half, in pretty good health, is affected in the following manner. From the age of nine years he has observed that his food comes back to the mouth after being swallowed, without having undergone scarcely any change; this generally takes place in about half an hour. He swallows it again without a sensation of either pleasure or disgust. He can, if smoking, or by an exertion of will, prevent this singular ascent from taking place, but then he soon finds that pain occurs in the epigastric region. He is always hungry, eats with pleasure both animal and vegetable food, but has a slight preference for the former. His drinks also come back to the mouth but give him no pain.—*Annales de la Med. Phys. Avril.*

4. *State of the Eyes in Amaurosis.*—The eyes of a person affected with amaurosis were examined by Professor HEUSINGER, and both optic nerves were in a state of atrophy, though the eyes were well formed and of the natural consistence. The sclerotic contained a considerable quantity of yellow coloured humour. The choroid and retina natural. The vitreous humour had disappeared, the lens was yellow and opaque. Three translucid vesicles as large as a pea and of a pyriform shape adhered to the outer surface of the retina. They contained a fluid similar to that which escaped in puncturing the sclerotic, and which Professor H. thinks was contained in similar vesicles. He thinks they were formed by Jacobs' membrane.—*Lancet, May 10th.*

5. *Spontaneous Combustion.*—Professor RUDOLPHI relates a case in the Journal de Pharmacie, of a man who suddenly felt a pain in the arm as if he had received a blow from a stick, and immediately perceived at the same spot a small flame which burnt his shirt. We extracted for a former number of this Journal the case of a young girl whose finger took fire. It is said that the best electrometers gave no evidence of electricity when placed near her finger.—*Lancet, May 17th.*

6. *Case of Amnesia.*—Dr CHAILLY in the Journal Générale, relates the case of M. Simon about 57 years of age, who was playing at the game of *tric-trac* in a warm room, felt a sudden pain in the left temple, and at the same moment lost the power of expressing his thoughts; his memory betraying him every time he attempted to use a substantive word, all words of that kind being replaced by *sonnez & six-cinq*, terms used in the game of tric-trac. He was taken home, and Dr CHAILLY being called, found him up, his face flushed, and complaining by gestures, of a pain in the left temple, but unable to say further than *J'ai là un sonnez.* Every thing was *sonnez* or *six-cinq*, with him. He

called his daughter by one of these names. He was bled both generally and topically, and used derivatives, by which he recovered.—*Nouvelle Bibl. Med. Avril.*

7. *On the Functions of different parts of the Ear.*—Dr Charles L. Esser, in a work crowned in 1825 by the Medical Faculty of the University of Bonn, has been led by numerous experiments to the following conclusions.

The *cartilage of the external ear* seems to contribute little to rendering sounds more clear, but seems to augment their force not only by reflecting into the auditive canal a part of the sonorous rays, especially those that fall into the concha, but also by means of vibration which the sonorous rays produce in it, and which it transmits to the tympanum.

The *bones of the head* contribute no less than the cartilage to the propagation of sounds. This propagation does not depend solely on the nerves as was thought by Treviranus and Swan, &c. for a watch applied to a swelled cheek ought to produce sounds as clear through the medium of the facial nerve, as if applied on the zygomatic arch, which is not the case. The os occipitis is better fitted for propagation of sounds than the bones of the anterior parts of the head which depends on its connection with the labyrinth, and its nearness to the mastoidal cells. The use of those cells is not to prevent an echo in the internal ear as M. Treviranus supposed; that function devolves solely on the eustachian tube. In many animals the bones that surround the external ear are disposed so as to favour in a high degree the transmission of sounds, and they offer compensation for the absence of an external ear.

The external auditory canal is evidently the part which contributes most to the concentration and transmission of sounds to the membrana tympani.

The membrana tympani is made to vibrate by the sonorous rays that reach it; nevertheless this is not the only use of the membrane, for the sonorous vibrations not only may reach the ear without the aid of the membrana tympani, but may even reach it with greater strength: it serves to protect the ear from external injuries.

The eustachian tube is the chief auxiliary to the membrana tympani, and fulfils four different functions. 1. It allows the air contained in the cavity of the tympanum to be in a state of equilibrium with the external air. If this equilibrium be lost, certain anomalous sensations are experienced, such as tinkling and buzzing of the ears. If the quantity of air in the cavity is augmented by strong expirations, then too strong a pressure on the membrane of the tympanum takes place as well as on the other parts of the cavity, particularly the *foramen rotundum:* this pressure produces *buzzing*, which decreases in proportion as the atmospheric equilibrium is restored through the eustachian tube. If the air of the cavity becomes rarefied, and the eustachian tube closed by spasm, then the external air pressing on the membrane finds its way through the pores of that organ, and thus produces the tinkling of the ears.

Both these phenomena disappear as soon as the equilibrium is restored in the cavity, and this may be effected by forcing the air along the eustachian tube by breathing with the nose and mouth closed, or by introducing the end of the little finger deeply into the external meatus and withdrawing it by degrees, making at the same time pressure against the upper surface of the canal. In this way a vacuum is formed, the membrane is brought back towards the external meatus and the eustachian tube gives passage to the air that presses into it from the fauces. This explanation manifestly can only apply to the occasional occurrences of buzzing, &c. Chronic examples of these affections depend on cerebral congestions or anomalous action of the nerves.

2. The second function of the eustachian tube is to admit of the occurrence of vibrations in the cavity of the tympanum, which could not be the case if it were entirely closed.

In cases of deafness occasioned by obliteration of the eustachian tube, perforation of the membrane becomes a means of cure by re-establishing the communication with the external air.

The idea that the sides of the tube are constantly in contact with each other is incorrect.

3. The tube prevents confused vibrations of the air of the cavity by giving them a passage outwards.

4. Finally, it serves to lead into the posterior nares the mucus secreted into the *cavity* and on its own surface.

The small bones of the ear, by means of their muscles, are capable of rendering the tympanum more or less tense; but, it is difficult to conceive why and how this effect takes place. Their influence on the sense of hearing is not very remarkable. They serve to transmit the sonorous vibrations of the membrana tympani to the foramen ovale, although this is not their sole use.

The labyrinth, whose anatomical history is sufficiently understood, is still, and will probably continue to be the obscurest part of the apparatus of hearing, as relates to its physiology. The experiments of WEBER leave it a doubtful question whether the lymph of COTUGNI exists during life, or is a production occasioned by the death of the animal.

The vestibule or the membranous bags which represent it in certain animals, and the semicircular canals appear to contribute most to the sense of hearing; but it is difficult to say in what manner. Some facts in comparative anatomy seem to shew that the principal use of the semicircular canals consists in strengthening the sounds. They are larger in animals whose external ear is less favourably formed or wholly wanting—large semicircular canals are generally accompanied with a small cochlea and *vice versa*.

The cochlea appears to be of less importance than the semicircular canals, for it soon disappears in the descending scale of animals, and birds only possess a rudiment of it. Its use appears to be, to offer a larger surface to the sonorous vibrations, and to strengthen by concentrating them.

The act of distinguishing different objects is a purely intellectual function which we must not seek for in any particular parts of an organ of sense. This power has been supposed to reside in the cochlea; but if its devolopment was in every case to be regarded as a measure of this distinguishing power, then the following order might be established. The cabiais and porcupine whose cochlea has three turns and a half; the dog and fox with three turns; man, the cow, the hog, and cat with two and a half turns; the horse and the dolphin with two and a quarter; and the hare with two turns: birds would occupy the lowest rank in this series. Such facts need no comments.

The part which the *acoustic nerve* plays in the sense of hearing is undoubtedly of great importance, but its manner of acting will always remain an impenetrable mystery.

The author of this interesting memoir finally arives at the conclusion that all parts of the auditory apparatus concur in the act of sensation; but the sense of hearing itself is not explicable by means of these parts, for like all the other senses this is purely intellectual. The soul alone sees and hears; the rest is both blind and deaf. *Bulletin, April.*

8. *Five Children at a Birth.*—A Russian peasant woman who had before had twins was delivered of five children in November 1824; to wit, she gave birth to a daughter on the 9th, 10th, 12th, and 13th respectively, and to a son on the 16th of the month. The children were each about eight inches in length. The son was born dead; the daughters died on the 6th day: as for the mother, she was restored to health at the end of a month.

III. PATHOLOGY.

9. *Dr* J. A. SMITH *on Medical Theories.*—Dr SMITH has published, in the New York Medical and Physical Journal, an eloquent "lecture on Medical Philosophy ;" on which we are led to offer some remarks, from the great influence which the acknowledged weight of his character must possess with a large portion of the American profession. Dr S. appears to have been already misunderstood, as he states in his introductory note; and indeed we think, under these circumstances, a small explanatory addition, by way of guarding his readers from the same errors into which his hearers had fallen, would not have been amiss. With the general course of his opinions we can fully concur. All that he says of the folly of hypothesis, and the necessity of a close adherence to fact and observation, is most true and of the highest importance. All that we have to object to, therefore, is what we conceive to be an erroneous application of these principles.

Dr S. appears to start with the *hypothesis* that the present age is similar to the last; and that, because former medical theories have been conjectural and perishable, it follows that the same will be the case with the doctrines which have supplanted or are supplanting them, at the present day. Thus, at page 176, the words " membranes, surfaces, tissues, and contrastimulants," are termed " the cant phrases of the day, and the legitimate successors of excitement, excitability, state of the system, &c." and to discourse with fluency of them is pronounced to be " very easy, very captivating, very imposing, and very useless ;" and these considerations are viewed as differing widely from the collection of dry details, the methodical arrangement of these, and the ultimate deduction of inferences by patient and continued thought. The subject of contrastimulism we, for the present, will waive ; but with regard to membranes, tissues, surfaces, &c., we are really ignorant where the facility of hypothesis described in these sentences is to be met with : certainly not in the vast collections of anatomical and physiological facts made by BICHAT, nor in those elaborate series of cases and inferences presented to us in the practical writings of LAENNEC, BROUSSAIS, LALLEMAND, and ABERCROMBIE ! To convict the solid medicine of modern times of being conjectural and hypothetical, would, we apprehend, be a difficult task, and require a more extended analysis of it than that presented by Dr S. The phrases, too, which he has designated as cant, possess at least this advantage over their predecessors, that they express something *tangible*, something with regard to which there is much more facility in demonstrating and establishing facts, than in the former. One can grasp a " membrane," a " tissue," or a " surface," in his fingers, or in a forceps, and hold it up to the light of the sun ; while it would not be easy to do the same by " excitability," or " the state of the system." The colour or thickness of a " tissue" is a *fact*, capable of being made apparent to all who possess the sense of sight in sufficient perfection, with sufficient knowledge of the subject to compare what they see ; while the state of " excitability" is a matter of opinion about which men of the highest attainments may differ.

Yet, notwithstanding this preference, we are of opinion that the doctrines to which Dr S. refers, as gone by, are not without their value in true medical philosophy. We think they cannot with justice be considered as overturned : they are only confined within their proper limits. Are we to suppose that the reflections of a man of genius and an observer are devoid of all utility, because he extends them to cases which, in consequence of human fallibility, he does not understand ? The very doctrines here alluded to are things which have their foundation in nature ; and we have only to guard against an improper extension of them. The age is in reality progressive ; the experience and reflections of one man are capable of being added to those of another, and human science is, in medicine, as in every other branch, the product of the labour of many minds.

The real truth we apprehend to be this. The presumption that the present age and its opinions are like those of former times, is *erroneous*. That portion of time which has elapsed since the American Revolution is distinguished from all others by a rapidity in the extension of demonstrable science unexampled in the history of civilized man. Within that period sciences which were previously in existence have been tripled in extent, while numerous new ones have been created, of which our forefathers never dreamed. Zoology, botany, mathematical astronomy, chemistry, geography, the science of government, have undergone extensions and accessions far beyond the value of their whole previous amount. Electricity, galvanism, electro-magnetism, meteorology, mineralogy, geology, political economy, the knowledge of the polarization of light, the discovery of the Egyptian hieroglyphics, all these and more, so numerous that memory refuses to furnish their names, without a longer warning, are among the glories of the present age. This is the epocha of constitutions and steam-boats, of London Universities, of United States and public opinion. There never existed such a period; and that which most nearly approached to it, the time when Greece was free and independent, was also distinguished for mental improvement and discovery. "We live," says RUSH, "in a revolutionary age; and our science has caught the spirit of the times." In our own department of knowledge, great advances have surely been made in physiology and surgery; and the admission of this ought to suffice, were the fact not directly demonstrable, to rebut the a priori supposition that no solid advance had been made in matter of such deep concern as medical doctrine.

We conceive that the improved extent and solidity of medical doctrines at the present day, is owing to the same leading cause which has produced the truly surprising results in public affairs which we have above enumerated. This is the *increased number of cooperators.* Revolutions in governments were got up by inducing the whole mass or the great majority of a nation to attend to its affairs; and circumstances of a similar character operate in medicine. Never before were there so many educated physicians employed in endeavouring to improve their science; nor did there ever exist such numerous and rapid means of communication. The immense multiplication of medical books, and, in particular, of medical journals, disseminates the knowledge of new facts to a larger number of individuals than at any former time; and there are, in consequence, more numerous competitors for discovery, and more frequent critics on each others' errors and extravagancies. That which makes a strong impression on public medical opinion at the present day, may be safely depended on for containing a far larger proportion of fact, and a smaller share of absurdity than common in former ages. The authors of new opinions know that they will be more freely canvassed, and that themselves need never aspire to become the objects of that idolatry which was so freely bestowed upon the great names among their predecessors. And no small portion of the credit of thus augmenting useful human sciences will be found to belong to the much debated name of Dr BROUSSAIS. We freely acquit Dr SMITH of any willingness to furnish ignorance and indolence with a convenient excuse for not studying new opinions, which it may be too laborious to investigate; but let him consider the influence which his ideas will have upon the minds of others; and he will perhaps fear to add to those difficulties which a foreign language, prejudices of education, and a laborious occupation throw in the way of the diffusion of modern improvements among our countrymen.

While the above journal is still in our hands, we observe that Dr T. ROMEYN BECK has stated, at page 201, that he "had not been able to obtain any authentic account" of the Pennsylvania Hospital, as an asylum for the insane. We are surprized at this; and would refer the now celebrated Medical jurist to the Philadelphia Literary Gazette, 1821, No. 15; to CHAPMAN's Philadelphia Journal, Vol. IX. page 30; to a Description of the Hospital, published in the present year, by Mr WILLIAM G. MALIN, clerk to the institution; for one of a peculiar documentary character, to that rich and valuable collection, HAZARD's Register of Pennsylvania, Vol. II. Nos. 6 and 7; and to the printed annual accounts of the establishment.

10. Dr ABERCROMBIE *on Apoplexy.*—In despair of being able to find space for a review of Dr ABERCROMBIE's invaluable *Researches on Diseases of the Brain,* and in consideration of our having already inserted one on the same subject, from the writings of LALLEMAND and BOUILLAUD (Vol. IV. page 106.),

we have thought it best to notice the one first mentioned in this summary. A part, at least, of that matter which we might have compiled from Dr A. is rendered superfluous by the article to which we have above alluded. Dr ABERCROMBIE's work is well calculated to support, in this respect, the reputation of the English school; abounding in cases, (too many, it seems, for the taste of some of the reviewers,) in well ascertained facts, and in careful deductions; and displaying an acquaintance with the labours of the continental physicians certainly beyond that of some of his countrymen, and which, if Mr CHARLES BELL retain the sentiments he once expressed, must, we should think, be truly distressing to him.

Dr ABERCROMBIE divides apoplexies into three species or forms: first, those which are immediately and primarily apoplectic; secondly, those which begin with a sudden attack of head-ach, and pass gradually into apoplexy; and thirdly, those which are distinguished by palsy and loss of speech, without coma.

"In the first form of the attack, the patient falls down suddenly, deprived of sense and motion, and lies like a person in a deep sleep; his face generally flushed, his breathing stertorous, his pulse full and unfrequent, sometimes below the natural standard. In some cases convulsion occurs, in others rigid contraction of the muscles of the extremities; and sometimes contraction of the muscles of one side, with relaxation of the other. In this state of profound stupor, the patient may die after various intervals, from a few minutes to several days; or he may recover perfectly, without any bad consequence of the shock remaining; or he may recover from the coma, with paralysis of one side. This paralysis may disappear in a few days, or it may subside gradually, or it may be permanent. Other functions, as the speech, may be affected in the same manner, being speedily or gradually recovered, or permanently lost; and recovery from the apoplectic attack is sometimes accompanied by loss of sight.

In these cases, where they prove fatal, Dr ABERCROMBIE has found either extravasation of blood or effusion of serum, or as in some instances, no morbid appearance whatever. The latter species he chooses to designate as *simple apoplexy*; and, after various discussions, comes to the following conclusions:

"1. There is a modification of apoplexy which is fatal, without leaving any morbid appearance that can be discovered in the brain.

"2. There is another modification, in which we find serous effusion, often in small quantities.

"3. The cases which are referrible to these two classes are not distinguishable from each other by any such diversity of symptoms as can be supposed to indicate any essential difference in their nature.

"4. Without any apoplectic symptoms, we find serous effusion in the brain in an equal or in a greater quantity than in the cases of the second modification.

"5. It is therefore probable that, in these cases, the effusion was not the cause of the apoplectic symptoms.

"6. It is probable that the cases of the first modification depend upon a cause which is entirely referrible to a derangement in the circulation in the brain distinct from inflammation.

"7. It is probable that the cases of the second modification are, at their commencement, of the same nature with those of the first; and that the serous effusion is to be considered as the result of that peculiar derangement of the circulation which constitutes the state of simple apoplexy. In other words, it is probable that the affection which has been called serous apoplexy is to be considered as simple apoplexy terminating by effusions."

The second form of the disease begins with a sudden attack of pain in the head; the patient becomes pale, sick, and faint, generally vomits, and frequently, though not always, falls down in a state resembling syncope; the face pale, the body cold, and the pulse very feeble. This is sometimes accompanied by slight convulsion. In other cases, he does not fall down; the sudden attack of pain being only accompanied by a slight and transient loss of recollection The patient continues for some time, perhaps an hour or two, cold and feeble, with cadaverous paleness of the countenance; his pulse weak, and generally frequent. He is quite sensible; but oppressed. By degrees, he recovers heat and the natural appearance of the countenance; and the pulse improves in strength. The face then becomes flushed; he is more oppressed; he answers questions slowly and heavily; and at last sinks into coma, from which he never recovers.—Death took place, in three cases of this form, at the respective periods of twelve hours, forty-four hours, and five days.

From the above description, fatality would seem to be a necessary character of the present variety of disease. Dr A., however, does not mean it so—these cases are *generally* fatal. He considers them as depending upon the immediate rupture of a vessel, without any previous derangement of the circulation; the rupture arising from disease of the artery at the part which gives way. The author compares them to those cases of concussion of the brain, in which the patient recovers from the first shock of the injury, but sinks under a subsequent bleeding, to be either relieved by the trephine, or to pay the forfeit of his life. Dr A. supposes that the brain undergoes a temporary disturbance of its functions, as the immediate effect of the arterial or venous rupture; and that this is recovered from; when, finally, either the bleeding progressively accumulates to an amount capable of suspending the actions of life by its *pressure*, or the vascular rupture is at first closed by a coagulum, for a time, but bursts out afresh and is rapidly fatal. The effusion of blood has, in some cases, been traced to arterial trunks; in others, to the rupture of the sinuses, of small aneurisms, &c.; while in another set, our author subscribes to the French opinion familiar to our readers, that the blood is frequently poured out by *exhalation*, without any rupture. He has found ossifications disposed in a singular series of long rings, separated by small intervening portions of the common appearance. This occurred particularly in elderly people.

At the ossified part, there is frequently a contraction of the cavity of the vessel; and sometimes a total obliteration. In other cases, the inner coat of the artery has been much thickened and softened; and capable of being pressed out from the divided extremity by the fingers.

In the third species, a singular example of the imperfections of our existing nosologies, an *apoplexy* distinguished by *palsy without coma*, Dr A. includes cases which resemble each other in their symptoms, but differ widely in the morbid phenomena observed in dissections.

" The attack appears under various forms; the most common of which is hemiplegia with loss of speech. But in some cases the speech is not affected; while in others the loss of speech is at first the only symptom. In some cases, again, one limb only is affected; which is commonly the arm.—Numerous other modifications occur; as palsy of one eyelid, or of the orbicularis of the eye, double vision, loss of the power of swallowing, &c. The patient frequently appears quite sensible of his situation, and makes anxious attempts to express himself; generally understanding, in a great measure, what is said to him, and answering by signs. Sometimes, however, when he retains a partial power of speech, his answers are incoherent and unintelligible. In the farther progress of cases of this class, we observe remarkable varieties, which may be chiefly referred to the following heads:—

" 1. Such an attack may be merely the prelude to the apoplectic, and may pass into it, after a short interval. These cases belong chiefly to the second class.

" 2. The attack may, under the proper treatment, pass off speedily and entirely, leaving, after a very short time, no trace of its existence.

" 3. The recovery may be very gradual, the use of the affected limbs being restored after several weeks or months.

" 4. The palsy may be permanent; that is, the patient, after a certain time, may recover so far as to be able to walk about, dragging his leg with a painful effort, and to speak very imperfectly, and, after this, makes no further improvement to the end of his life; which may be protracted for many years.

" 5. In a fifth variety, the patient has no recovery; he is confined to bed, speechless and paralytic, but possessed of his other faculties in a considerable degree; and dies gradually exhausted, after several weeks or months, in some cases without coma, in others with coma a few days before death.

"In endeavouring to investigate the morbid conditions of the brain which are connected with these varieties, we find considerable diversities, which, in a pathological point of view, may be referred to the following heads:

"1. Many of the cases appear to have a close analogy to simple apoplexy; and, when they are fatal, present either no satisfactory appearance, or only serous effusion, often in small quantity.

"2. Extravasation of blood of small extent, contained in defined cysts in the substance of the brain or under the membranes.

"3. Ramollissement of the cerebral substance.

"4. Inflammation and its consequences."

Our author's account of the absorption of blood from the apoplectic deposit does not materially differ from that given by the best modern pathologists. Indeed he tells us that he has not found the cysts to be ultimately obliterated, as "the French writers believe." This is admitted, it is true, by Dr SERRES; but we had understood it to be the prevalent French opinion that apoplectic caverns did not become obliterated. Certainly such is the expressed belief of Drs MARTINET and ROCHOUX, the only writers of this class whom we have at hand. There may, however, be foundation for it, as, to do the French anatomists justice, they have generally appeared to us to be characterised by superior knowledge of the subjects of which they treat, rather than by their entertaining any particular set of opinions. So far from "the French" being a body of men who maintain an exclusive code of opinions, we have thought that there existed among them quite as much discussion and difference of sentiment as in other nations.

Dr A. describes softening of the brain (why does he use the foreign term "*ramollissement?*") in nearly the same manner as his fellow-labourers of the continent. Vide the Review already referred to. He compares it to gangrene, and refers it to *inflammatory action*, or to the *failure of the circulation* depending upon disease of the arteries. His cases differed from M. ROSTAN's; not being confined to patients of an advanced age. In cases of palsy, Dr ABERCROMBIE has met with the eight following morbid alterations:—

"1. Simple and recent inflammation of the cerebral substance.

"2. This inflammation passing into softening.

"3. The encysted abscess of the brain.

"4. Induration of a portion of the cerebral substance.

"5. Extravasated blood in the ventricles, on the surface of the brain, and in cavities or cysts in the substance of the organ.

"6. The empty cysts from which extravasated blood has been absorbed.

"7. Serous effusion on the surface of the brain.

"8. Extensive disease of the arteries of the brain."

Dr A's *treatment* is exceedingly simple; and we are affected with both pleasant and unpleasant emotions at finding that his experience, enlightened as it is by a clear and certain pathology, has led him to the same conclusions which have been forced upon a large number of practitioners by the facts of daily observation alone. We are glad to find clinical induction confirmed, while we are sorry that there is no very important accession made to the curative means. Still, however, we should not feel disappointed in our hopes of gaining useful practical results from this more close study of the disease. By means of an improved diagnosis, we may succeed in applying and apportioning well known and simple remedies with more discrimination and consequently more success; a power, which, rather than an extended acquaintance with drugs, or even a large and indigested mass of routine experience, constitutes the difference between the skilful and the commonplace practitioner.

"We have also seen grounds for believing that the cases which terminate by effusion are probably, at their commencement, in this state of simple apoplexy. We have seen, farther, that we have no certain mark by which we can ascertain the presence of effusion; and, finally, we have found, that even extensive extravasation of blood in the brain may be entirely recovered from by the absorption of the coagulum. These considerations give the strongest encouragement to treat the disease in the most active and persevering manner. They teach us, also, not to be influenced in our practice by the hypothetical distinction of apoplexy into sanguineous and serous; and finally, not to be hasty in concluding, in any particular case, that the disease has passed into a state in which it is no longer the object of active treatment.

"In the treatment of apoplexy our remedies are few and simple. Those in which our chief reliance is to be placed, are, large and repeated blood-letting, active purgatives, and cold applications to the head, aided by an elevated position of the body, cool air, and the absence of all stimuli. Antimonials may occasionally be useful as an auxiliary, from their known effect in restraining vascular action; provided, in the early stages, they do not occasion vomiting.

"Our first great object is to take off the impulse of blood from the arteries of the head, by bleeding carried to such an extent as shall powerfully and decidedly affect the system, and by repeating it, at short intervals, as soon as these effects begin to subside. The first bleeding should probably be from the arm; but, after this, there seems to be an evident advantage in abstracting blood locally either from the temporal artery or by cupping. Much importance has been attached by some to bleeding from the jugular vein, as most likely to give immediate relief to the head; but we must remember that the only jugular vein which can be opened, is the *external* jugular, which has very little communication with the brain; and consequently, that bleeding from it is probably much inferior to bleeding from the temporal artery."

We would here ask, in what respect is the communication between the temporal artery and the vessels of the brain closer than in the case of the external jugular vein?

"As soon as possible after the bleeding, means are to be taken for inducing strong purging. The most efficient remedy, by far, for this purpose, is the croton oil; and, if the patient cannot swallow, it may be very conveniently introduced into the stomach, suspended in thick gruel or mucilage, through an elastic gum tube. The operation should be expedited by strong purgative injections. This is always to be considered as a most important and leading point in the treatment of apoplexy; and though, in arresting the progress of the disease, our first reliance is upon large and repeated bleeding, the first decided improvement of the patient is generally under the influence of powerful purging. The effectual application of cold to the head is the third remedy on which we rely, and it is equally applicable to the different states of the disease, whether arising from simple apoplexy, or from extravasation. It may be applied either by means of cold water, or pounded ice in a bladder, or by a full stream of water directed against the crown of the head, and received in a basin held under the chin, while the patient is supported in a sitting posture. I have formerly given an example of a patient restored in a few minutes or rather seconds by this remedy from a state of perfect apoplexy.

"The active use of these remedies is sometimes followed by a very speedy removal of the apoplectic state. In other cases, though little immediate effect may be produced, yet, by a persevering repetition of them, the coma begins to subside after some time; perhaps a good many hours, or even several days. In other cases, again, they may be used in the most active manner without relieving the patient; and, after all, we may find, upon dissection, that the disease was still in the state of simple apoplexy."

Dr A. proceeds to recommend vigour and perseverance; and mentions a case in which upwards of a hundred ounces of blood were abstracted, upwards of twenty drops of croton oil given, and the case terminated favourably, after a very doubtful state of three or four days' duration. He thinks that there is no class of symptoms which characterize a species of apoplexy, in which the treatment should be essentially changed; none, for example, which hinge upon the supposition of a *serous apoplexy* caused by debility. On reference to his cases, he finds weakness of the pulse and paleness of the countenance to be frequent symptoms of the worst form of sanguineous apoplexy; and, on the other hand, that cases which were accompanied by strong pulse and flushing of the countenance have terminated by serous apoplexy. In one remarkable case, there existed every circumstance that could lead him to consider the disease as serous apoplexy, but the patient died, without any effusion; while, in another, there was most extensive effusion without any apoplectic symptom. He here adds the remark that "in apoplexy the strength of the pulse is a very uncertain guide; for that nothing is more common than to find it upon the first attack of apoplexy, weak, languid, and compressible, and becoming strong and full after the brain has become in some degree relieved by large blood-letting."

11. *Instrumental Percussion of the Thorax.*—The invention of a mode of making percussion produce a louder sound, by means of a small piece of wood, a contrivance of Dr P. A. Piorry, an Adjunct of the Faculty of Medicine at Paris, is probably known to many of our readers. Dr P. has now produced a volume upon the various applications of which this instrument is capable; and has furnished a surprising number and variety of these. They form a natural and valuable appendage to the copious means of analysis afforded us by the happy researches of M. Laennec; of which they are, in numerous instances, only an extension. Not satisfied, however, with the investigation of the diseases of the thorax, M. Piorry has directed his attention to the employment of the same instrument over the remaining portions of the body; as Drs Kergaradec, Lisfranc, and others have done by the stethoscope.

Laennec had called the use of his invaluable instrument by a phrase which has been translated "*mediate* auscultation;" and the expression of Dr Piorry is called "percussion médiate." Reference to a dictionary will show that the above English expression is incorrect; as *mediate* does not signify acting *by* a means, but acting *as* a means.

The instrument of our author is called by him a "*plessimetre*"; from πλήσσω, I strike, and μέτρον. A Hellenistic friend furnishes us the remark, that it should have been taken from the aorist ἔπληξα, and not from the present; which would give us, in English, *pleximeter*.

We present these verbal criticisms, in themselves unimportant, from an aversion to a species of servile copying too prevalent among our journalists and other writers, which leads them to adopt the modes of expression used among our European contemporaries, without inquiring whether they are either correct in the original, or consonant to the genius of our own language. It is thus we see "M. such-a-one," used instead of Mr, or what is more proper Dr. This practice is an acknowledgment of inferiority. A manly avowal of the real fact, when it is actually the case, is both honourable and philosophical; but we see no reason for degrading the language in a manner which seems to imply that it is unfit for scientific use. The English is capable of any application; and foreign words, where necessary, should be translated or anglicised, following the Anglo-Saxon spelling, or, where this is inconvenient, the Roman, which harmonizes much better with our own than that of any modern nation. We could offer various other remarks on medical barbarisms; but forbear on account of the occasion, and ought to apologize for so occupying thus much of the Quarterly Summary. The *pleximeter* is not described by the reviewer, on the ground that "it is in every body's hands." Whether the instrument be the same which is in our hands, we are thus rendered unable to determine. The latter consists of a circular piece of wood, about two inches wide, and fastened to a small handle, placed obliquely. The flat piece is applied against the thorax or other part to be examined, for the purpose of being tapped on by the fingers.

Dr Piorry's opinions may be divided into two classes; those which he considers himself as having demonstrated by observation, and those which he believes himself authorized to infer from analogy, and from the nature of the process described, we give a sketch, compiled, with some care, from the review in the "Nouvelle Bibliothèque Médicale."

In hydrothorax, the fluid is naturally found in the lowest portion of the thorax, in whatever position the patient is placed for examination. When not of sufficient volume to fill the whole pleura, the existence of the fluid may easily be detected by attention to this principle. When the patient sits erect, the lower part of his thorax emits, on percussion, a sound obviously duller than natural; and this extends, in every direction, as high as a horizontal line easily ascertainable by this mode of inquiry. When the patient lies on his back, after a few moments of rest, the whole anterior edge of the thorax, which is now uppermost, recovers its natural sonorous state, and sounds hollow when struck; while the duller sound is produced all along the posterior part. Again, a corresponding change is produced by placing the patient on his face, or on either side; the part left uppermost being found sonorous, and the lowermost, to which the fluid has subsided, becoming otherwise. In every instance, the line of demarcation is distinct and horizontal. Now, in pneumonia, in which the substance of the lungs is rendered thick and hard, a similar dull sound is produced; but we have here no change in the position of the part which yields it, and no horizontal line of demarcation between the healthy and the diseased.

Where the dropsy fills the whole pleura, and the duller sound is heard over the whole surface of the thorax, on one side, it is extremely improbable that the disease can be mistaken for pneumonia; because the latter disease has hardly ever been known to affect the whole of a lung. This has however sometimes occurred; leaving a deficiency in this part of the diagnosis.

The sound produced where there exists a pleuritic effusion of moderate depth, is not what is called absolutely dead; but forms a tolerable medium between that emitted by tapping over the heart, and that exhibited by sound lungs subjected to the same trial. When the quantity of liquid at liberty in the thorax is very small, it must be sought for, not only at the lower or abdominal extremity of the cavity, but at the posterior part, next the vertebral column.

Inflammation of the lungs, then, almost always presents inequalities of outlines, by which it may be distinguished from pleural effusion. Of apoplexy of the lungs, (the local effusion described by Laennec), the same remark holds good. A dead sound obtained in the regions immediately above or below the clavicles furnishes a *very constant* sign of tubercles. A gradation of sound a little less clear on one side of the thorax, when observed in the situation just mentioned, affords a suspicion of numerous miliary tubercles. A sound less clear over the whole of one side than on the other, or any very obvious differences of sound between different points in the same side, if, at the same time, the symptoms of pneumonia are wholly wanting, "may produce a belief" of the existence of tubercles scattered generally throughout the lungs, and separated by portions of sound lung.

When there exist large, empty excavations, situated near the surface of the lung, surrounded by tuberculous or hepatised portions, and, around these again, by healthy lung, the percussion yields a tympanitic sound, of loudness proportioned to the dimensions of the cavity. This sound is not detected in very small excavations. If the cavity be half filled with fluids, and there exist open communications with the bronchia, there will be a "*humoral* sound," capable of being easily distinguished. The same phenomenon is produced when the cavity is placed at a considerable depth.

For the purpose of studying diseases of the heart, percussion with the pleximeter should be combined with feeling and the use of the stethoscope. The author here gives some opinions which the reviewer treats as hypothetical, or, at least, as requiring further observations to test their correctness. If the dull sound produced by percussion over the heart be heard for a space exceeding two inches, [in diameter?] and the pulsations of the organ can be felt over the whole surface, Dr Piorry considers the heart as enlarged at its point and anterior surface. If the pericardium contain much liquid, the dead sound is very distinct on the left side. Tumours of the aneurismal character, it is thought, can be detected; and if covered by a thin stratum of lung, a slight tap will produce the hollow sound; while a harder blow sounds dull.

In the *abdomen*, our reviewer expresses himself dissatisfied with Mr Piorry's results in regard to affections of the peritoneum lining the muscles, in tumours, and particularly in hydatids. These indications are omitted in his analysis. In effusion in the cavity at large, the same observations may be made which we have recorded of the thorax when under the same circumstances. The stomach yields a very clear "son stomacal"; and the intestines a sound intermediate between the latter and that of the liver; which last yields "son jecoral." To the remarks which relate to position should be added the consideration, that when the quantity of fluid in the abdomen is very small, it collects entirely within the cavity of the pelvis, from which it must be made to issue by elevating the patient's hips; when, according to the part placed lowest, it may be detected beside the vertebræ, on either side, or along the front of the belly.

Encysted dropsies yield a dead sound, far more dull than that of fluid rolling at large among the intestines. It may also be recollected, that the sound of the latter is more exclusively dull at the lower part; and yields some sonorous effect when struck at the upper part where intestines may be supposed to be floating partly immersed within it.

We candidly confess we are unable to understand the description given in the review of the situation and extent of the jecoral sound. We should prefer searching for it by our knowledge of the situation of the liver. Indications of an obvious nature must be expected, when the liver is greatly increased or reduced in size. The reviewer thinks that small enlargements or diminutions could not be discovered in this way. Dr. P. conceives that he can distinguish the spleen, but there is great uncertainty about this organ. By thrusting the front of the abdomen upwards, an assistant can elevate the liver and stomach, and with them the spleen; and this leaves the kidneys uncovered, in which Dr P. proposes to discover enlargement by means of his instrument. We confess we should despair of success in such an attempt.

In cases of peritonitis, Mr Piorry recommends the employment of a very broad pleximeter, to be struck with great precaution, from a fear of bruising the inflamed membrane.

Percussion will demonstrate the distended bladder by its dull sound; if, at the same time, the patient be placed in different attitudes, and the dulness be found, notwithstanding, to continue in the same place.

The affections of the *uterus* he postpones for future inquiry; and a field is here opened for other experimenters.

IV. THERAPEUTICS, MATERIA MEDICA, AND THE PRACTICE OF MEDICINE.

12. *The Conservation and Reproduction of the Medicinal Leech.*—We gladly borrow an article, having the above title, from the July number of the London Medical Repository, in the confident belief that it cannot fail to interest our readers at this present time when the practice of free and frequent leeching is becoming so general in this city and other parts of the United States, in not only the phlegmasiæ, nosologically so called, but in all febrile diseases.

M. Chatelain has published two treatises on this subject (*Annales de la Méd. Physiol. Jan.* 1827, *et Journal de Physiologie Experimentale*, tome vii. 1827). His observations confirm the supposition that the leech is androgynous, and that the generative process is performed by reciprocal and simultaneous impregnation. One leech is capable of producing three capsules *(cocons)*, but on an average, one capsule only is obtained from each individual, and nine leeches from each capsule. The author has observed where a plurality of capsules occurred, that they were formed at intervals. The young escape from the capsule within twenty-six or twenty-eight days.

Contrary to the opinion of Dr **Pallas** (*Journ. de Pharmacie, Juin*, 1827), M. **Chatelain** thinks sanguisuction is unfavourable to the fecundity of the leech. The plethora thus occasioned generally caused a mortality of one-third of his leeches in the space of two or three months, notwithstanding frequent change of water and diversity of situation. In order to effect disgorgement of blood, the leeches were usually subjected for four or five minutes to a solution of one part of common salt in ten of water. For their reception during the season which M. **Chatelain** found in Provence to extend from the latter part of July to the middle of September, stone pans or jars, each containing a sufficient bed of moist clay, were arranged in the open air, so as to be fully exposed to the sun. Two fine leeches were placed in each jar, which was then covered with a cloth of open texture to prevent escape without excluding all light. With the same view, M. **Chatelain** was accustomed to draw a brush, dipped in equal parts of sulphuric acid and water, round the inside of the jar, about four or five inches above the clay. The jars were visited daily, and care taken to keep the clay moist.

It is remarkable that the pairs could never be detected in the act of copulation. This was supposed to be accomplished within the burrows which these animals made in the clay.

The Preservation of Leeches.—M. **Pfeufer**, physician to the hospital of Bamberg, recommends the use of a wooden tub, furnished with a cock and a long funnel, for the purpose of renewing the water, which should be done with as little disturbance as possible. At the bottom of the tub he would have some mud from the pool whence the leeches were taken, with one or more roots of the calamus aromaticus or of the iris florentina. This was the method adopted by M. **Chatelain**, and is followed in most of the hospitals in France.

M. **Schutz** takes some ounces of fresh powdered charcoal, which he washes sufficiently with water; on this powder he pours a solution of twenty grains of sugar of milk in warm water. In this mixture he places his leeches, keeping them in a temperature between 50 and 55° Fahr. The water, as well as the solution of the sugar of milk is renewed every fortnight; great care being taken that the water be not too cold, and that the vessel be not much shaken.

M. **Hampe** uses small tubs charred on the inside, covered only with gauze. At the bottom is placed sand, and upon this some moss. River water only is employed, to which he adds a few lumps of charcoal. In summer the water is changed weekly, by means of a hole pierced immediately above the layer of sand. In winter it is enough to change the water once in six weeks, preventing the access of cold. This method is much recommended.

M. **Voget** also advises us to add to the water some of the soil to which the leeches have been accustomed; but the peculiarity of his method is the addition of the oxide of iron.—*Bulletin des Sci. Méd. Avril*, 1820.

For farther particulars on this subject, as well as on the manner of applying leeches for the abstraction of blood from the skin of the patient, we refer our readers to the Review of *Derheims on Leeches* in our first volume.

13. *Iodine in Gout.*—Doctor **Gendrin**, the able author of *L'Histoire Anatomique des Inflammations*, speaks in high terms of the curative powers of the iodine in gouty affections. He first employed it externally for the discussion of arthritic tumours. Encouraged by his success, he afterwards prescribed it in acute and violent gout with equally fortunate results. Several patients were completely cured by the medicine. In two only was the treatment difficult. The continued use of the iodine for two or three months after the complete cure of a paroxysm prevented its return.

Before giving in our adhesion to this new practice, we would like to know precisely the condition of the digestive passage as regards the sensations of heat, tenderness, spasm, or pain, the irritability of the stomach, the extent of the sympathy of the sanguiferous system, whether the attacks were chiefly articular or gastric. These points determined, we ought next to be apprised of the dose by which to begin the treatment.

14. *Bloodletting in Peripneumonia in Children.*—Dr CUMING, assistant physician to the "Institution for the diseases of Children" in Dublin, gives the following directions in regard to the quantity of blood and the manner of abstracting it in the cases under consideration:—

"In a child about two years of age, from three to four ounces may be abstracted; and when the age is above four, about five, six, or eight ounces may be drawn, according to circumstances. In an infant under six months, though general bloodletting may often be required, the application of three or four leeches to the back of the hand or foot will for the most part answer the purpose where a vein, which is frequently the case, cannot be found. It is better to apply the leeches to the hand or foot than to the thorax, for, when they are applied in the latter situation, it is difficult to stop the bleeding after they have fallen off; and instances have occurred in which a fatal hemorrhage has been the consequence of a continual oozing from four or five leech bites. When they are applied to the hands or feet, the bleeding can be easily stopped by placing compresses of lint over the bites, and securing them, as after the operation of V. S. by a bandage. As far as my observation goes, leeches applied to the extremities are nearly as efficacious in removing local inflammation in infants, as when applied in the vicinity of the parts affected. They seem to produce the effect of a general bloodletting: as the face and lips become pale, the pulse faulters, and syncope occasionally takes place followed by vomiting. These effects are apt to be produced when general venesection is carried to a considerable extent, and sometimes a state of nervous agitation and general commotion is induced, which, if not speedily removed, may terminate in death. The best remedies in a case of this kind are the horizontal position, cool air, and a drop or two of the tincture of opium."

When we hear, as in this instance, of an experienced physician recommending three or four leeches to the back of the hand or foot as an adequate means of drawing blood from an infant under six months attacked with peripneumonia, we on this side of the Atlantic are bound not to deride his practice, but rather to admit that the European is a much more formidable animal than the American leech—the more especially since instances are said to have occurred of fatal hemorrhage from the application of three or four of the former to the thorax of infantile subjects; an occurrence never thought of among us. Our leeches are not such desperate sanguisuckers, otherwise we could hardly dare to apply, as was done by the writer of this article, ten leeches to the neck of an infant eight days old, for bronchitis approaching in some of its symptoms to croup. The warm bath and antimonials had been used without effect. At times there seemed to be a complete suspension of respiration, and from all appearance the little sufferer could not have lived two hours longer unless decided relief were afforded. The leeches procured this relief, and, from a few hours after their application until the present time, the child has been perfectly healthy. Within these few days we directed the application of twelve leeches over the epigastrium of an infant seven weeks old for violent pertussis. No hemorrhage was feared or ensued after the bites of these animals; nor was there any notable debility or languor induced by the loss of blood which they had abstracted. They were, however, vigorous and drew well.

15. *Cases in which the Moxa was successfully applied.*—Such is the heading of a paper by R. WADE, member of the Royal College of Surgeons, and inserted in the Med. and Phys. Journ. for June 1828. In the case of epilepsy cured by this means, the moxa was applied at first in three places, viz. one over the junction of the occiput with the atlas, and one to each side of the neck, and subsequently in two places daily for a period of six weeks; and every third or fourth day during three months, with an interval between these two periods of six

weeks, during which time the patient (a female aged seventeen years) was in the country.

The case of paralysis was of a female aged fifty-seven. The muscles of the right side were quite useless, and scarcely any indications of returning sensation were observed. Other remedies failing, a moxa was burnt slowly over the deltoid, and another over the biceps; a slight redness of the skin being produced by their combustion. On the very next day there was more sensation in the arm, and the patient was able to lift it three or four inches from her side, although on the preceding day she had not the slightest power over the muscles. Four moxas were applied on this day—one to the deltoid and three to the glutos. The application of the remedy was continued to different muscles of the body for about five weeks, with the effect of enabling the patient to walk with the assistance of crutches. The use of the flesh brush, and as much exercise of the muscles as could be borne without fatigue, completed the cure.

16. *Bromine and the Hydro-bromate of Potassa in Scrofula and Goitre.*—We read in the *Ephémérides Médicales de Montpellier* for May an account of the medical properties of these substances, by Mr POURCHE. Bromine, regarded by some of the most authoritative chemists as a simple body, is found in sea-water. It is fluid at the common temperature of the atmosphere, and even at 8° F. below zero. The first case was of scrofulous tumours of a large size on the neck of a young woman aged twenty-two years. M. P. prescribed six drops of bromine in three ounces of water—to be taken in the course of the day in three doses. On the following day four drops were directed in the same quantity of water. After the lapse of ten days the daily allowance was increased by four drops. From this time the patient experienced, after taking a dose of the medicine, but only during five or six minutes, some flushing in the face, a slight cephalalgia, and a dryness of the throat and thirst. The quantity of the bromine was augmented in a month from its first administration to thirty drops daily. Cataplasms of crumbs of bread moistened with a small quantity of the bromine were likewise applied to the tumours. Perseverance in this course for three months was productive of a removal of the tumours, with the exception of a hard and large tubercle, the resolution of which is not hoped for by the physician.

In another case of scrofula marked by tubercles of the neck and ophthalmia of one eye, the other having been lost by scrofulous inflammation, M. P. prescribed the hydro-bromate of potassa internally in the form of pills, and externally as a pommade. The patient took at first two pills of a grain each daily, and frictions were exercised on the part three times during the same period with a pommade composed of one drachm of hydro-bromate of potassa and an ounce of lard. On the sixth day of the treatment four pills were taken, and on the eighteenth, six, and on the thirty-second, eight. This course, persevered in for three months, accomplished a complete removal of all the scrofulous symptoms. The only evidence of excitation from the medicine was a slight increase of temperature for two or three minutes after the ingestion of the pills.

The third case was of chronic otorrhœa, with a hard tuberculous swelling of the epididymis of each side, the latter consequent on gonorrhœa, but for which mercury had been administered without effect. The patient had suffered under an attack of tubercular phthisis of which he had been *cured* by four moxas and the use of preparations of gold. The hydro-bromate of potassa was, on the present occasion, prescribed internally in the form of pills, and the bromine applied externally to the parts. For the first ten days, the patient took four pills of the hydro-bromate of potassa, of one grain each; during the ten days following, six; and for two months. eight pills daily. Cataplasms, moistened with a solution of bromine, were applied to the scrotum, and injections of bromine, thirty drops to six ounces of water, were thrown into the external meatuses. This treatment was continued for a period of three months, after which both the otorrhœa and the indurated epididymes had entirely disappeared.

The fourth case was of goitre, the treatment of which by the bromine was completely successful. The author thinks that this new agent is entitled to a higher rank in the class of remedial agents than iodine.

17. *Intermittent Facial Neuralgia cured by Sulphate of Quinia, used according to the Endermic Method.*—Dr Caucanas relates the history of this case in the *Journal Universel des Sciences Médicales* for May. The subject was a lady, aged thirty years, who, after exposure to a cold and moist air, was seized with acute pain of the left ear, which soon extended over the whole left side of the face. Various remedies were put in requisition to allay and remove this malady which made its attack every evening. Repeated leeching the part —opiates—the carbonate of iron were all tried in vain. During the administration of this last mentioned article the stomach became excessively irritable. The periodicity of the disease seemed to indicate the use of the quinia, which could not however be given in the usual manner on account of the state of the stomach. On a vesicated surface on the arm, therefore, fifteen grains of the sulphate of quinia, mixed with some cerate, were applied. The day was passed tranquilly—no paroxysm during the night, and seven hours of sleep obtained. A similar application on the following day gave a night of entire tranquillity. Two more days of treatment were sufficient for an entire cure.

18. *The Warm Bath in Intermittent Fevers.*—Dr Gassaud of Calvi has published, in the *Nouv. Bib. Méd.* for June, some observations on the advantages derived from the use of the warm bath in intermittent fevers. It is, as he justly remarks, a very ancient remedy, sanctioned by the recommendation of Hippocrates himself. Of the cases which Dr G. adduces as illustrative of its efficacy some are not of a very confirmatory nature, since the cure would seem to have been finally brought about by very reduced diet and abstinence from all tonics and stimuli, and even in two instances by the application of leeches to the epigastrium. He recommends the patient to be immersed in a bath of blood heat on the day of the fit, and to remain in the water as long as his strength will allow.

19. *Cases of the Poisonous Operation of Iodine.*—One of these was of a lady, twenty-six years of age, of a nervous and irritable habit, who, wishing to put a period to her existence, swallowed two drams and a half of the tincture of iodine. The immediate effects experienced were dryness and burning of the throat, lancinating pains of the stomach, nausea, efforts at vomiting; the face was flushed, eyes watery, pulse small and contracted; the pains of the epigastrium were exasperated by the slightest pressure; convulsive pains of the upper limbs.

Vomiting was excited by glassfuls of warm water taken at intervals; the ejected matter was yellow and had the odour of iodine. An anodyne draught was then administered and an opiate plaster applied to the epigastrium. Small enemata of a decoction of poppy heads were also used at the same time. All the unpleasant consequences were soon removed and yielded to a refreshing sleep.

Dr Gendrin, editor of the *Journal Général*, in commenting on this case, tells us that of all the preparations of iodine the tincture is the one which can be least tolerated in large doses by the patient. We may, however, by gradual progression in the course of a few days, carry the quantity as high as one dram to a dram and a half of the tincture in the twenty-four hours. The solution of the hydriodate of potass is much more readily borne, and can after a time be taken in very large doses. Dr G. was giving at the time he wrote (in June) an ounce of this preparation daily to a lady labouring under very painful chronic gout. On the third day of the administration of the medicine the pain was diminished: on the eighth it had ceased entirely, although for two months preceding it had persisted uninterruptedly; the swelling of the joint is rapidly diminishing.

Another case related in the Journal above mentioned is of M. A. Chevallier, who suffered from violent colics on two different occasions by exposure to the vapours of iodine.

M. Raspail, on the other hand, cites his own personal experience of the impunity with which the vapour of iodine may be inhaled for some hours in a room filled with it.

20. *Melancholy with Aphonia cured by the Actual Cautery.*—Dr Rossi relates, in the *Repertorio di Torino* for January last, the case of a man thirty years of age, of a robust constitution and sanguine temperament, affected with profound melancholy and complete aphonia. Various methods of cure had been resorted to in vain: and the cauterisation of the vertex by iron of a white heat was finally determined on. The action of this agent extended to the bones. The patient was better on the very next day after the application, and could answer questions in a low voice. In proportion as suppuration was established the intellectual faculties were restored. Purgatives were then had recourse to; the posterior part of the neck was covered with blisters, whilst the anterior was rubbed with ammoniacal liniments. The infusion of arnica was given internally with the addition of camphorated ether. Ammonia was inhaled by the patient from time to time. At the end of two months he had recovered his health.

We should be tempted, notwithstanding the caption of this article, to ask by what means the disease in this subject was finally removed. If cauterisation were relied on, why so prodigal in medication of other kinds.

21. *Ammonia in Drunkenness.*—Dr Vatteteau communicates some cases in the *Journal Universel* for April, in which the stupor and other unpleasant and even alarming symptoms of drunkenness were speedily removed by the ingestion at short intervals (ten minutes) of liquid ammonia, in doses of six to twelve drops, mixed with sugar and water or some such simple drink. Occasionally he directed the volatile alkali to be applied to the nose of the drunken person and to be inhaled by him. Sulphuric ether was also administered with equal success in doses of twelve to fifteen drops in a suitable vehicle. Two or three doses of the ether, making in all thirty to forty drops, usually suffice to restore the drunken man to the use of his senses and intellect; and to enable him to move about with readiness.

22. *Erysipelas cured by Cauterisation.*—A young soldier of the royal guard had a spreading erysipelas of the lateral, anterior and posterior parts of the chest, consequent on the application of caustic potass to the soft central part of a swelling on the right axilla. The eschar was excised, but erysipelas supervened, and was only arrested by slightly touching the affected surfaces in many points with iron of a brown heat.

23. *Hydrochloruret of Lime as a Remedy in Fever and Dysentery.*—Dr Reid gives several cases (*in the Transactions of the Association of the Fellows and Licentiates of the King and Queen's College of Physicians in Ireland*) of the good effects of this medicine in the above mentioned diseases. In one instance, in which the patient was recovering from fever, a relapse was brought on by his friend stupidly though with kind intention giving him some wine to drink. " 2d April.—In the night he complained of most violent pain of the head. He lies nearly insensible. Low muttering delirium. Tongue deep yellow, dry and furred; great subsultus.

Half an ounce of the mixture, with hydrochloruret of lime and tincture of Columbo, was directed to be given to him every second hour.

3d. Much better in all respects; general warm perspiration, not profuse, but still some delirium and subsultus.—Continue medicines.

4th. Tongue cleaning, subsultus gone. No delirium.

5th. Still continues to improve. Tongue dry, and he cannot put it outside his teeth, though he apparently makes considerable effort when desired to do so. I directed a blister to his neck, and to continue the medicine as before.

6th. Much fetid discharge from the blister. Continues to improve; can now freely put out his tongue when desired. It is now moist and clean at the edges. He has some appetite to day.

7th. Still improving. The diaphoretic mixture was now substituted for that with the hydrochloruret.

12th. Up and dressed, sitting on his bedside. Well.

This was one of the most remarkable cases (says Dr Reid) I have witnessed of the efficacy of the hydrochloruret; whether it be considered with reference to the severity of the disease, or to the rapidity of the patient's recovery."

A case is related by Dr R. exhibiting the influence of the hydrochloruret of lime in controlling disease of the pulmonary organs resulting as he says from febrile excitement.

In dysentery, the hydrochloruret was used both by the mouth and with enemata. The latter was found a very effectual means of correcting the fetor of the stools which at times amounted to an intolerable stench.

The mixture directed by Dr Reid was ten grains of the hydrochloruret of lime with two drachms of the tincture of Columbo, in four ounces of water and two drachms of syrup; half an ounce to be taken every hour or two.

24. *Treatment of the Epidemic Dysentery which prevailed in Dublin in* 1825.—Dr O'Brien, in his paper on this disease contained in the Transactions of the College of Physicians in Ireland, gives the following as the plan of treatment which he adopted and which appeared on the whole to prove most successful. "The patient, if young and in the first stage of the disease, was immediately blooded to the extent of twelve or fourteen ounces; he was then placed in a warm bath at a pretty high temperature, in which he was retained for fifteen or twenty minutes, if possible, but the harassing necessities of the disease seldom permitted so long an interval of quiescence. He was then, after a vigorous friction of the whole body, but particularly of the abdomen, rubbed with camphorated oil, put into bed, having been previously swathed with a flannel roller, drawn tightly round the abdomen and loins, in order to support the bowels, a measure which is always attended with comfort and alleviation of pain to persons labouring under this disease. After this a small bolus, or pills containing ten grains of calomel, one or sometimes two grains of opium, and occasionally combined with antimonial powder, were administered, and repeated after an interval of eight hours, if the urgency of pain rendered such repetition necessary, for the anodyne effects of opium scarcely extend beyond this period. As patients are generally conveyed to the hospital at a late hour in the day, the measures just described occupied the first evening and night after admission. On the morrow a purgative draught, consisting sometimes of castor oil, tincture of rhubarb, and tincture of opium; sometimes infusion of senna and salts, and sometimes the neutral salts alone. The author has not found from experience that castor oil possessed more irritating qualities than the other purgatives above mentioned, and thinks that the prejudices which some practitioners entertain against it are altogether groundless; but all apprehension on this subject may be removed by the addition of tincture of opium, which the author thinks will even promote its purgative virtue in the disease before us. If the symptoms still continued urgent, and the pulse gave indication of the presence of inflammatory and febrile action, the bleeding and warm bath were repeated, and the calomel and opium continued as above, alternated however by purgatives until the disease yielded, or until ptyalism, or, at least, soreness of the gums was effected. When this latter effect was produced, which in some instances was accomplished with considerable difficulty, the mercurial part of the medicine was discontinued, and the opium alone exhibited, accompanied as before by the alternate use of purgatives. In many instances the Dover's powder was substituted for opium and achieved the cure with equal success, and indeed this modification of opium gained confidence in proportion to the frequency of its employment, and cannot be too highly commended in this disease."

Of the patients under Dr O'Brien's care, about one in four died, and he considers this number as a fair average of the ordinary mortality from dysentery in the Dublin Hospitals.

The duration of the disease when it terminated favourably varied from two to six weeks. The discharges from the bowels were of a dark brown colour, exhibiting insulated patches of blood floating on the surface. The seat of pain was, generally, the prominent part of the hypogastric region, a little below the umbilicus. The colour of the skin was in every instance altered to a darker hue, assuming, as the complexion varied, either a leaden or a copper tinge; and in general it affected the touch with the sensation of roughness and even corrugation. The eyes were anxious, the brows wrinkled, the countenance dejected, sunk, pale, and cold; the features sharp and pointed.

The post mortem appearances in twelve cases were as follows: "The liver was found diseased in six; the spleen in three; small intestine (chiefly ileum) inflamed or ulcerated on its mucous surface in eight; great intestine diseased in twelve; gangrenous in one; much contracted in two; ulcerated and inflamed in all; colon and rectum parts most diseased."

It must be very difficult after the evidences thus furnished of the seat of the malady and of the tendency to disorganization of the parts, together with the symptoms, for a physician of any observation to withhold the use of the lancet and topical means of depletion by leeches, and, where the abdomen is not too tender, by cups. Dr O'Brien does not seem to include topical bleeding as one of the curative agencies in dysentery: he is silent also as to the efficacy of enemata, whether of cold water in the earlier stages, or of mucilaginous fluids and opium combined in the more advanced periods of the disease. And yet, if we were required to choose, we should have no hesitation, arguing from the sporadic cases of dysentery which annually come under our notice, to give the preference to venesection general and local, diluents and opiates by the mouth and enemata, over any and every other kind of medication by vomiting, purging, sweating and blistering. Even if purging were thought admissible we should have great misgivings about allowing our patients to take purgative tinctures, as of rhubarb, or any preparation of senna, which however prepared is so apt to excite violent griping.

25. *Suppression of the Urinary Secretion for more than seven weeks, cured.*—One of the German journals contains the history of a case of this nature recorded by Dr De Racum of Riga. The subject of it was a child twelve years of age, in whom the secretion of urine was suppressed without occasioning any derangement of the other functions, or without any auxiliary or supplemental process of secretion being carried on during the period above mentioned. The introduction of a silver catheter into the bladder was not followed by the discharge of a single drop of urine. After attempting various other methods ineffectually Dr De Racum had recourse to the following preparation, which restored the renal secretion on the third day. Purified oil of amber, two drams; Venice turpentine, ten drams; balsam copaiva, one ounce. Thirty drops to be taken thrice a day in emulsion of almonds. In addition to this he directed friction with oil of turpentine twice a day on the lumbar region; and a regimen vegetable and diuretic. The author assures us that the above remedy is equally efficacious in gravel.

26. *Good Effects of Cold Affusion.*—In the fourth number for July of Dr Drake's *Western Journal of the Medical and Physical Sciences*, we find related a case of *ischuria* successfully treated by the cold affusion. The patient had at the time a dysentery. He was placed in a tub of tepid water, and bled freely from the arm, but without any relief to the suppression of urine now of forty-eight hours duration. Dr Lewis Campbell, his physician, who is also the narrator of the case, still allowing him to remain in the bath, took a half gallon pitcherful of cold spring water, and poured it in a continued stream on the region of the bladder and pubis. Before the third pitcher had been emptied, the urine broke forth in a full and free stream. The same remedy was had recourse

to on the four following days—other treatment adapted to the dysentery being employed during this period. After this the convalescence was speedy and uninterrupted.

The next is a case of *poisoning by opium* treated with success, by the affusion of cold water, by Dr John F. Brown of Tennessee. The patient had swallowed opium at two different times the preceding evening: the first piece made him vomit, and he repeated the portion, which, from his description, must, Dr Brown thinks, have weighed two drams. Fourteen hours elapsed from the time of his taking the opium until the physician arrived. All attempts to provoke vomiting by the forced ingestion of tartar emetic and sulphate of zinc and tickling the fauces proving ineffectual, it was resolved by Dr Brown, in consultation with Drs Young and Snider, to have recourse to cold affusion. The patient was accordingly laid on the floor, and a large vessel of cold water poured on him, commencing with the face and directing it to the breast. The second vessel produced some struggles, and so soon as the third was beginning to be poured on him, he suddenly raised himself on his seat, and said "I cannot stand every thing." The cold affusion was continued, and so soon as sensation was restored, vomiting was produced by titillating the fauces. Strong coffee was next freely given. In about two hours the emetic medicines produced catharsis. Frictions and moderate exercise, with some other gentle remedies, were then recommended.

In the evening, when Dr Brown repeated his visit, he found his patient cheerful and apparently free from the effects of the opium.

27. *Obstinate Constipation relieved by Injections per Anum.*—The August number of the London Medical and Surgical Journal (formerly the London Medical Repository) contains a short paper by Dr Epps, one of the editors, the chief object of which is to recommend a perseverance in the employment of enemata to relieve obstinate constipation. He gives the case of a gentleman who had taken calomel to the extent of producing salivation, saline draughts, manna and rhubarb mixtures, and castor oil, with the view of opening his bowels, but all to no purpose. The colon and cæcum were found by Dr Epps distended with feculent matter, which, together with the medicines that had been taken, had induced so much irritation that the least touch on the abdomen, in the situation of the cæcum, induced the most pungent pain. Leeches were ordered to be applied over the parts most painful, and an injection of castor oil to be given, an injunction being added, that should it not operate, common barley water injections were to be exhibited every hour until an effect was produced. The castor oil enema did not operate, and it was not until sixteen injections in the whole had been administered that the bowels were opened. In a week or two the patient perfectly recovered. Dr Epps adverts to the good effects of this remedy in children in whom the most unpleasant symptoms have been relieved by it.

V. MEDICAL JURISPRUDENCE.

28. *To determine, approximatively, a long time after Death, whether the Individual died from Poison, and to ascertain the nature of the Poisonous Substance.*—Such are the questions to solve which have been the aim of Professor Orfila and Dr O. A. Lesueur, in their memoir on the subject. Our knowledge of it is chiefly derived from the initial article in the *Journal de Chimie Médicale, de Pharmacie et de Toxicologie, Juin,* 1828; and from this we propose drawing on the present occasion.

The conclusions of these learned medical jurists rest chiefly on the two following kinds of experiments:—

1. Mineral and vegetable poisons in varying doses were dissolved in a pint of water, and mixed with animal matter; the mixture was exposed to the open air in

vessels with a large opening, a portion of the fluid contents being analysed every eight or ten days.

2. The same poisons mixed with albumen, gelatin, flesh, &c. were enclosed in stomachs and intestines, which latter again were put in deal boxes, well closed and buried at the depth of two and a half feet. After the lapse of several months these boxes were dug up, and their contents analysed.

On the other hand, again, human bodies, buried in deal coffins at the depth of four feet under ground, have been disinterred one, six, ten, and even seventeen months after inhumation, in order to ascertain as near as may be the epoch at which vestiges of the digestive canal are still discoverable, and to exhibit the presence of a poisonous substance which may have been introduced into the stomach or intestines during life. This last inquiry, which is to be the subject of a work by MM. ORFILA and LESEUR, on *juridical exhumations*, has led them to the discovery that for many months, and even for years after death, when a soft part can be no longer recognised, there is found on each side of the vertebral column, in the abdominal region, a kind of brownish *coom*, which is evidently the remains of the digestive canal, and in which a portion of the poison can have remained either altered or in its natural state.

The poisons which constitute the subject of the present memoir are the sulphuric and nitric acids, and arsenious acid (white arsenic), corrosive sublimate, tartar emetic, sugar of lead, proto-hydrochlorate of tin, sulphate of copper (blue vitriol), nitrate of silver (lunar caustic), hydrochlorate of gold, acetate of morphia, hydrochlorate of brucia, acetate of strychnia, opium, and cantharides. These were examined in succession, as follows:—

Sulphuric acid (oil of vitriol).—It is possible to be assured of the presence of *concentrated* sulphuric acid several months and even years after its admixture with animal matters; all that is necessary is to treat the mass in which its existence is suspected, with tincture of turnsol, or hydrochlorate of barytes, or metallic mercury. If the sulphuric acid have been much *diluted* and mixed with substances, which by putrefaction disengage ammonia largely, it is saturated by this alkali to such an extent that there is none or hardly any of it free at the end of some months. In this case we are no longer authorised to conclude that there has been poisoning by means of sulphuric acid; at most we are at liberty to infer, supposing the sulphate of ammonia to have been obtained in a crystallised and well marked form, some *probability* of poisoning, as this sulphate does not *commonly* constitute a part of the alimentary matter, nor of those composing the digestive canal. The best means of ascertaining the presence of a small quantity of *free* sulphuric acid in the cases in question would be to treat the liquid at a common temperature with *pure* sub-carbonate of lime, freed from all sulphates: there will be formed, in the course of a few seconds, sulphate of lime if there be free acid, whereas none will be produced if the liquor only contains saturated sulphate of ammonia, since this salt is not decomposed at a low temperature by the subcarbonate of lime until after the expiration of a certain time.

Nitric acid (aqua fortis).—*Concentrated* nitric acid may be detected many months after its mixture with animal matters, and even when putrefaction is at its height. For this purpose it is sufficient to treat the liquor by potassa, then to evaporate it to dryness and to throw a portion of the product on live coals; another part mixed with metallic copper and sulphuric acid diluted with a very small quantity of water will furnish vapour of an orange yellow colour. If the nitric acid have been considerably *diluted* and employed in small quantity, the ammonia, which results from the putrefaction of the animal matters, being more than sufficient to saturate all the acid, we can at most merely prove the fact of the presence of nitrate of ammonia, which does not *necessarily* suppose that there had been poisoning by means of the nitric acid, since the nitrate just mentioned might in truth be formed from all the parts during the putrefaction of the animal matter.

Arsenious acid (white arsenic).—We have it in our power to show the presence of arsenious acid which has been mixed with animal matters, after the expiration even of several years. We must, however, in many cases, in order to succeed in this, free it from the greater portion of these matters by evaporating to dryness the liquid which contains the poison, and agitating for some minutes the product of the evaporation with boiling distilled water; the solution then exhibits the same characters with hydrosulphuric acid (sulphuretted hydrogen) which the pure arsenious acid would do. We cannot too carefully direct attention to the fact, that, by its combination with animal matters, the arsenious acid thus dissolved is so far disguised as *not to become yellow* even when treated with hydrosulphuric acid, but it is sufficient to have recourse to the simple method above indicated to render its presence fully evident. If the writers, who have proposed more complicated processes, have not detected arsenious acid in the liquids ejected by vomiting and in the fluid contents of the stomach and intestines, it is because they have not pursued the course indicated above, and more especially they have neglected uniting these liquids with hydrosulphuric acid and a drop of hydrochloric (muriatic) acid, whereas the deuto-sulphate of ammoniacal copper is an uncertain reagent, as already shown in the *Leçons de Médicine Legale*, 2d edition, tome iii. p. 112.

If the arsenious acid or white arsenic have been given in a solid state it will not be impossible even a length of time after inhumation to discover here and there grains, which being taken up on the point of a penknife will exhibit all the characters of the arsenical poison.

We cannot doubt but that the arsenious acid is after a time transformed, and, in proportion as ammonia is generated, into an *arsenite of ammonia*, which is much more soluble than the arsenious acid: hence it may happen that after the lapse of some years we are unable to demonstrate the presence of arsenious acid, where it would have been very easy to have detected it some months after inhumation, because this acid before solid and granular became so soluble as to have transuded through the pores of the coffin, or had flowed off through the holes which are often found on the lower surface of these boxes, when putrefaction has made rapid progress.

Though the arsenious acid employed in large quantities stops the putrefaction of animal matters, this is not the case when used in minor proportions.

Corrosive sublimate.—Corrosive sublimate dissolved in water is decomposed with considerable rapidity so as to render it impossible for us to detect its presence in the liquid matters after some days, except by means of a plate of gold covered in spiral fashion by another of tin, aided by the action of hydrochloric acid. There is more of the sublimate decomposed in proportion to the large quantity of animal matters made use of. It does not appear, however, that these matters can decompose all the corrosive sublimate, since by the aid of a plate of gold the experimenters succeeded at the expiration of *several hours* in procuring an atom of metallic mercury from a solution of six grains of the sublimate mixed with a *large quantity* of animal substances. In all cases we can, by treating, by means of heat and potassa, the animal matters which decompose the corrosive sublimate, obtain metallic mercury, even many years after the sublimate has acted on them. Now the presence of this metal, though it may not prove the existence of corrosive sublimate, indicates at least that of a mercurial preparation.

Tartrate of potassa and antimony.—Tartar emetic mixed with animal substances is decomposed after some days, so that the tartaric acid is destroyed and the oxide of antimony precipitated. Whence it results that it is impossible to exhibit this salt in treating it by the reagents usually employed to detect it; but we can obtain metallic antimony from solid matters even after the expiration of many months. The alteration in question is rather the result of the action of water and air on this salt than of animal substances; for experience proves that a solution of three grains of tartar emetic in a pint and a half of distilled water exposed to the air undergoes a similar decomposition, and that we can no more exhibit the antimonial salt in it at the expiration of thirty or forty hours in summer, than we could do if to a like solution we had added albumen or gelatin.

Acetate of lead (sugar of lead).—Some days are sufficient for the acetate of lead, dissolved in water and mixed with animal matters, to be decomposed to such an extent that not an atom of it is found in the liquid; but we can obtain metallic lead by calcining to a red heat the animal matters and the precipitate of a blackish gray which is formed in the liquid.

Proto-hydrochlorate of tin.—All that has been said of the acetate of lead will apply to this salt.

Sulphate of copper (blue vitriol).—Mixed with animal substances the dissolved deuto-sulphate of copper is decomposed, so that none of it is found, after the lapse of a certain time, in the liquid. This decomposition is not indeed so rapid that we are unable to find a portion of the salt in solution, even after the lapse of some months, if we operate on some drams of the deuto-sulphate. If we are called upon to analyse these matters at the epoch when it would be impossible to detect the coppery salt in the fluids, we must dry the solid substances and carbonise them in order to obtain the metallic copper, whilst another portion of the carbonated matter is to be treated with nitric acid in order to obtain the nitrate of copper.

Verdigris.—By being kept with animal substances under ground verdigris is decomposed and the deutoxide of copper is combined with the fat of the dead bodies so as to produce a kind of soapy matter insoluble in water. In a case of poisoning of this nature, it would be possible to exhibit the presence of the deutoxide of copper several months and even years after burial. All that is required would be to treat the saponaceous mass by the hydrochloric acid, which would dissolve the deutoxide of copper, or to calcinate in order to obtain metallic copper.

Nitrate of silver (lunar caustic).—Nitrate of silver in solution is rapidly decomposed when it is mixed with animal matters; so that we must probably endeavour to obtain this metal from the solid matters, if we are called upon to pronounce on the fact of a poisoning by its salt many days after burial. Experience shows that we can accomplish this purpose even at the expiration of several months.

Hydrochlorate (muriate) *of gold.*—A few days will suffice also to prevent our discovering the hydrochlorate of gold in a solution of the salt exposed to the air and mixed with animal substances; but by having recourse to calcination in the same way as for the nitrate of silver, we obtain metallic gold many months after inhumation.

Acetate of morphia.—It is possible to determine the presence of the acetate of morphia, in a case of juridical disinterment, many months after death. For this purpose we must operate not only on the liquid, but also the solid matters found in the stomach and intestines, because supposing even that the poisoning had been effected by a watery solution of the acetate of morphia, it might have been decomposed and the morphia in part precipitated. Numerous experiments have proved that the acetate of morphia merely dissolved in water and exposed to the air is not long in being *in part* decomposed; the acetic acid of the decomposed portion is destroyed, whilst the morphia is precipitated, if not entirely at least in great part. These results are in accordance with those obtained by MM. GEIGER and DUBLANC the younger. In a case of juridical exhumation, indeed, if the poisoning had been effected by a watery solution of the acetate of morphia, there would be found less morphia precipitated than we should at first suppose, because a part of that deposited would be redissolved by the ammonia formed during putrefaction. We know in fact that in precipitating the morphia by ammonia in a somewhat dilute solution of the acetate, it is sufficient for us to shake the precipitate for a few minutes with a mixture of ammonia and water to *re-dissolve* it.

In order to obtain the morphia which may exist in the solid matters, we must at first treat these latter with frequent additions of alcohol, then evaporate the alcoholic solution, and expose the product of the evaporation to water with a slight addition of acetic acid; without this latter precaution, it would be difficult to separate the morphia from the fat of the dead body, which is *abundantly* formed while under ground. Should the liquor by chance be coloured, it will be rendered colourless by heating it with *purified* animal charcoal, and filtering it several times through this same body, without having recourse to the sub-acetate of lead and the hydrosulphuric acid, the employment of which appears useless.

In comparing the action of the nitric acid and the test of hydrochlorate of iron on the matters which constitute the subject of these experiments, it is easy to perceive that the nitric acid has constantly reddened them, even though they had been a little coloured previously, whereas the chalybeate salt only *blued* them in general in as far as they had been perfectly discoloured; and again in certain cases it developed a reddish colour though these matters were colourless.

It would be very censurable were we to pronounce *affirmatively*, in a case of juridical exhumation, that there had been poisoning by a preparation of morphia, because we had observed *only* the two colorations, *red* and *blue*, of which we have just spoken. The evidence from these changes is only presumptive. It would not be so if we obtained many months after death, as happened to MM. ORFILA and LESEUR, a whitish crystallised substance, insoluble in water and sulphuric ether, fusible in a mild heat, becoming *red* by nitric acid, and *bluish* by the salt of iron, and exhibiting in a word all the known characters of morphia: we ought not under such circumstances to hesitate *affirming* that the suspected matter is *morphia*.

Hydrochlorate of brucia.—The presence of brucia and hydrochlorate of brucia may be rendered evident in the digestive canal of the bodies of persons poisoned by these substances, and buried for many months. But here, as in the case of the acetate of morphia, the phenomenon of coloration, developed by the nitric acid and the proto-hydrochlorate of tin, ought not to be considered as evidences of poisoning; and we must, in order to be able to *affirm* positively the fact, separate the brucia and determine their peculiar properties.

Acetate of strychnia.—This salt has been recognised in solution many months after having mixed it with animal matters and water,—the vessel containing it being exposed to the air. In other respects the means of analysis employed do not differ from those commonly put in practice to exhibit the presence of the preparations of strychnia.

Hydrocyanic (Prussic) *acid.*—We learn from the experiments of M. LASSAIGNE that it is not possible to display, by chemical means, the presence of small quantities of hydrocyanic acid three days after death; the disappearance of the poison depends on the decomposition which it undergoes.

Opium.—There is greater difficulty in detecting morphia in a body disinterred, into the digestive canal of which opium had been introduced, than if we had to deal simply with a salt of morphia. This difficulty is at times insurmountable; hence the authors of this memoir are of opinion, that in no case must we pronounce *affirmatively* of the fact of poisoning by opium, unless we can recognise this substance by its *physical* and *chemical* properties, or obtain morphia which shall exhibit all the characters already indicated. Nor must we even in this latter case conclude in a positive manner, that poisoning has taken place by opium, but rather by opium or morphia, or acetate of morphia.

Cantharides.—A dram of pulverised cantharides has been introduced into a portion of small intestine, with meat and albumen; the intestine was tied at both ends and put into a thin deal box, which was then buried at the depth of two and a half feet. The exhumation of the box took place after nine months and five days, and it was easy to see at this time, that the intestine had been converted into the fat of dead bodies. Scattered through this substance could be seen, even with the naked eye, a multitude of brilliant points of a *superb green*, formed by the powder of the cantharides. By treating the mass with boiling water, the fatty matter was melted, and came to the surface of the liquid in the form of an oily coat, while the shining particles were seen at the bottom of the vessel. A sufficient quantity of these particles could also be collected to enable the experimenters to determine that they possessed all the properties of pulverised cantharides.

MM. ORFILA and LESEUR conclude their memoir by introducing a reply to the following question. The poisons which have been detected as above in different disinterments had, some will say, been placed in contact with the organs only after death. Can we then conclude that they would have been found on investigation of the bodies of persons killed by poisoning? The reply is *affirmative*, provided at the time of death there was in the digestive canal a quantity of poisonous substance appreciable by chemical means. Of what moment is it after all whether the action of a poison on our tissues be different during life and after death? Of what avail for us to know that a portion of this poison has been absorbed or rejected with the matters vomited, or passed by stool during the life-time of the individual? The capital point for us to be apprised of is that the quantity of the venomous substance which the medical examiner could discover twenty-four hours after death, can be also ascertained ten, fifteen, or twenty months after interment. Now there cannot from the above detailed results or experiments be any doubt on this head, since these venomous substances cannot act or be acted on in a different manner in the digestive canal of a body buried, from what they were in the stomach and intestines in which they had been enclosed, after their having been mixed with alimentary matters.

VI. SURGERY.

29. *Old Luxations.*—In the seventh volume of the Philadelphia Journal of the Medical and Physical Sciences, professor GIBSON has recorded the case of James Scofield, æt. 50 years, who had his left os humeri dislocated at the shoulder joint early in March 1822. About three weeks after the accident, an attempt at reduction was unsuccessfully made. On the 10th of May, two months after the accident, he consulted Dr GIBSON, and on the 12th, the reduction was effected at the Alms-House Infirmary by extension and counter-extension with bandages and pullies, after endeavouring to break up the adhesions by rotating the arm in different directions, and assisting the above measures by free bleeding from the arm. Half an hour after the operation, some swelling, attributed to inflammation, was noticed; this gradually increased without pain or uneasiness; the patient became pale and pulseless, and died in the evening.

Dissection exhibited a minute orifice in the subclavian vein, and a complete rupture of the axillary artery owing to the firm adhesion of this vessel with the capsule of the joint where it surrounded the neck of the bone. Much blood was effused.

In the third number of the American Journal of the Medical Sciences, Dr GIBSON records another unfortunate accident of the same kind. John Langton, æt. 35, of intemperate habits, applied for assistance nine weeks after a dislocation had occurred at the shoulder joint, and after several forcible attempts at reduction had been made. On the 6th of March, he was admitted in the Alms-House Infirmary, and four days afterwards was submitted to a decided antiphlogistic course; being bled daily to ℥x, kept on barley water, and purged occasionally. The shoulder was rubbed with hot oils and the arm gently moved several times a day, in various directions. On March 15th, reduction was accomplished at the expiration of — hours and — minutes by means of a counter-extending band fixed to a staple, and extending band attached to a pully, assisted by free bleeding and the use of tartrite of antimony to induce nausea. Next day, March 16th, there was a general swelling, but not more than usual on such occasions; it however manifested a distinct pulsation of an aneurismal character. March 17th, the aneurismal symptoms have increased. At 3 P. M. it was decided that the subclavian artery should be secured without delay. This was done by Dr GIBSON.

A detailed account of the case is given; the circulation did not completely return in the limb—the hand and forearm manifested symptoms of gangrene; the patient became prostrated and died.

Dissection.—The subclavian artery was found secured by the ligature near the scalenus; below, this vessel was found ruptured, owing to a firm and close attachment having existed to the os humeri and the articular capsule by dense ligamentous substance. From the appearances, there was some reason to believe that a small aneurismal tumour had formed, perhaps from previous attempts at reduction, which tumour was ruptured on the 16th of March, giving rise to a diffused aneurism. Considerable quantities of blood were extravasated, but were partially confined by the adhesions.

These cases are certainly unfortunate, especially as no human ingenuity could have previously detected the adhesion of the artery and bone. Similar accidents are exceedingly rare, and hence the best surgeons have unhesitatingly recommended forcible rotation of the bone to tear up its adhesions to the muscles, cellular membranes, ligaments, &c. Dr GIBSON is not able to find any very decided cases similar to his own, in the records of surgery. DESSAULT mentions that in one case, a large tumour, supposed at first to be sanguineous, and afterwards emphysematous, formed under the pectoral muscle immediately after reduction. This disappeared and the patient recovered. PELLETAN alludes to several cases, and mentions that tradition has preserved an account of a case as having occurred at the Hôtel-Dieu, in which a tumour formed after the bone was reduced. It was regarded as emphysematous and punctured—the patient died of hemorrhage. The only authenticated cases, however, collected by Dr G., have been lately (1827) published by M. FLAUBERT, [*Flanbert?*] chief surgeon of the Hôtel Dieu, at Rouen. Vide Medico-Chirurg. Rev. 11th vol. p. 452, and Ephémérid. Médical. de Montpellier, March 1828. In the first case, reduction was effected *eleven days* after the accident by M. LEUDET. The patient died on the 14th day after the operation; signs of ruptured artery, gangrene of the arm and hemorrhage from two openings below the axilla having previously occurred in regular succession. *Dissection* displayed the pectoralis major almost completely torn across the upper portion of the short head of the biceps, and the axillary artery was also ruptured. This vessel adhered to the rib by means of effused lymph. In two other cases tumours formed, followed by irritation, syncope, loss of pulse, &c. from which the patients narrowly escaped. One of these was a luxation of the fore arm backward; the second a luxation at the shoulder joint. Two examples of injuries of the *nerves* caused by forcible measures for reducing dislocated humours are also given by M. FLAUBERT. In one, the axillary plexus seemed to be injured. The patient recovered with a permanent paralysis. In another, a woman, æt. 70 years, had a luxation of the humerus for five weeks; a circumstance at first unknown to M. FLAUBERT. The reduction caused violent pain and induced symptoms of threatened suffocation, followed by pallor, nausea, coldness of the affected side of the body and even a transitory syncope, afterwards by a hemiplegia, which continued till death; this event occurred 18 days after the operation. *The nerves of the arm, adhering to each other, had been torn from the spinal marrow at their origin.*

30. *Sanguineous Tumours of the Female Mammæ.*—In the Ed. Med. and Surg. Journ. for July, Dr JOHN RODMAN has given an account of some collections of blood in the mammæ, the nature of which is obscure, and the event has sometimes been fatal. They appear to originate in the cellular, not in the glandular tissue of the breast; sometimes without any evident cause, occasionally as the consequence of slight mechanical injury. Three stages may be noticed. In the *first*, there is a gradual and moderate enlargement of the mamma, easily to be distinguished from the periodical fulness, which arises from uterine excitement, and also from any kind of inflammatory irritation, although subsequently a kind of inflammatory action of a chronic character may be excited, causing some firmness to be felt in a few cases of the disease. In the *second* stage, a tumour may be distinctly observed, as a small conical or oval body, not very moveable, having "a circumscribed pulpiness, and a kind of soft doughy elastic firmness, with a peculiar smoothness of circular and compressible exterior." Its uniformly ovoid character, its degree of mobility, its uniformity, and a notable obtuseness of feeling, with its situation in the cellular tissue, are diagnostic circumstances. This stage may be short, or, as in most cases, it may last for months or years. The

third stage is characterised by a great enlargement of the tumour from the accumulation of sanguineous fluids; giving rise to a painful sense of distention, enlarged cutaneous veins, thinning of the integuments, &c. The cyst in this stage is delicate, with little vascularity, and with slight attachment to the surrounding parts. The contents vary; consisting sometimes of a dark coloured grumous, thick, and tenacious bloody matter, often merely of a sanguineous fluid, and sometimes, no red particles are observed, a serous fluid only existing in the sac. Cases are given from authors and from Dr RODMAN's own practice, from the results of which he would recommend that during the *treatment*, attention should be paid to the stages of the disease. Pressure by bandages proves highly useful in the second and third stages, but less so in the first stage. The repeated evacuation of the contents, by punctures, not by incisions, when the tumour is bulky, in order to prepare for the modes of treatment, is advantageous in the third stage alone. Punctures, even very frequently made, are useful in relieving the distention, in preventing the bad consequences which would hence result, and in facilitating the contraction of the cyst and the consequent diminution of the diseased growth. The cure may often be hastened by injecting vinous or other fluids into the cyst as in the treatment of hydrocele. Extirpation in the second stage has been useful; in the advanced stage it is almost superfluous, as the gland disappears spontaneously. The general health is usually disturbed and requires appropriate treatment, as all local affections are influenced by constitutional disturbance.

31. *Tetanus.*—Mr EDWARD LEAH, assistant surgeon of H. M. S. Albion, records in the Ed. Journ. for July, two cases of tetanus on board the Albion after the action of *Navarino*. In one, tetanus succeeded a gunshot wound of the fleshy part of the arm on the seventh day, and proved fatal with great rapidity, although the system was brought effectually under the influence of mercury—and large doses of laudanum were freely given. In the second, the spasmodic disease began at a later period, after an amputation of the shoulder joint, rendered necessary by a severe cannon shot wound. The lower jaw was nearly closed for several days and spasmodic rigidity of the abdominal muscles occurred. The patient recovered under the moderate use of opium, and a careful attention to diet, regimen, and the general state of his health. His bowels were opened by mercurial and colocynth pills, occasionally repeated.—Laudanum in doses of 80 or 90 drops was prescribed at night, and the linimentum ammoniæ wi'h laudanum used to the jaws. Spoon meat was freely allowed. His mouth was, at one time, a little affected with the calomel. Port wine was allowed, the wound granulated and cicatrized, and the patient gained rapidly both flesh and strength.

Another case of recovery from traumatic tetanus occurred after the same battle, under the care of Mr SANKEY, surgeon of the brig Musquito. The arm was shot away close to the body, and the patient soon after had his jaw fixed; the muscles of the neck and abdomen quite rigid, and the spasms occasionally severe. Moderate doses of opium and camphor with the sulphate of quinia and nutritious diet were relied on with success.

32. *Amputation of the Lower Jaw.*—In the ninth number of this journal will be a notice of Mr CUSACK's cases of disarticulation of the lower jaw. Mr LISTON of Edinburgh has since reported a case of osteo-sarcoma of the right side, for which he extirpated that portion of the bone between the canine tooth and the angle. The patient apparently recovered in a few weeks, but, soon afterwards, the disease re-appeared in the upper remaining portion of the bone, and Mr LISTON successfully removed this fragment at the joint. The hemorrhage was arrested by a single ligature on the common trunk of the temporal and maxillary artery. The patient recovered with loss of motion, not of sensation, of the right side of the face.—*Ed. Med. & Surg. Journ. July*, 1828.

33. *Chronic Inflammation of the Joints.*—The July number of the Ed. Med.

and Surg. Journ. contains a review of a small octavo volume lately published by Mr John Scott of London on the treatment of Chronic Inflammation, especially of the joints. Perhaps there is nothing strictly new in the principles and practice here recommended, especially to those American surgeons who are acquainted with the uniform practice pursued for many years with great success by Dr Physick—*of keeping the diseased joint perfectly at rest* by means of suitable splints, and in pursuing a course of purging as the best method of causing revulsion. Mr Scott's method is somewhat different; his principles very similar.

Inflammation, involving the whole structure of a joint, has its origin usually in one of these tissues,—in the synovial capsule, in the cartilages, or in the bones, each of which may be generally detected. Eventually all become affected; lymphatic and purulent effusions occur, followed by abscesses, caries, ulcerations, and fistulæ, with great constitutional irritation. During the first stage, when there is much heat and pain in the joint, in addition to *absolute rest*, local bleeding, by cups or leeches, is very important, to be repeated on the continuance or return of these symptoms, provided, however, the constitutional powers are not greatly impaired; for, in such cases, Mr S. thinks local bleeding, instead of abating, aggravates the disease. The common bread and water poultice is the best application after the bleeding; cold lotions are not recommended, as they tend to repress rather than encourage the cutaneous circulation. When the above measures have been pursued sufficiently far, Mr S. would not resort to the usual modes of revulsion by blisters, issues, &c. &c.—not that they are invariably useless, but they are always uncertain in their operation, often aggravate the patient's sufferings, and in many instances directly increase the violence of the disease by their irritating character,—the inflammation excited by them being propagated to the articular textures. Mr S. therefore substitutes the following method. "In the first place, the surface of the joint, suppose the knee, is to be carefully cleaned by a sponge, soft brown soap, and warm water, and then thoroughly dried; next, this surface is to be rubbed by a sponge, soaked in camphorated spirit of wine, and this is continued a minute or two, until it begins to feel warm, smarts somewhat, and looks red. It is now covered with a soft cerate made with equal parts of the ceratum saponis, and unguentum hydrargyri fortius cum camphorâ. This is thickly spread on large square pieces of lint, and applied entirely round the joint, extending from at least six inches above and below the point at which the condyles of the femur are opposed to the head of the tibia; over this, to the same extent, the limb is to be uniformly supported by strips of calico, spread with the emplastrum plumbi of the London Pharmacopœia. These strips are about one inch and a half broad and vary in length; some are fifteen inches, others a foot, others half these two lengths, and the shorter or longer are selected, according to the size of the part round which they are to be applied. This adhesive bandage ought to be so applied as to preclude the motion of the joint, prevent the feeble coats of the blood vessels from being distended by the gravitation of their contents in the erect posture, and thereby promote their contraction. Over this adhesive bandage, thus applied, comes an additional covering of *emplastrum saponis* spread on thick leather, and cut in four broad pieces, one for the front, the other for the back, and the two others for the sides of the joint. Lastly, the whole is secured by means of a calico bandage, which is put on very gently, and rather for the purpose of securing the plaster, and giving greater thickness and security to the whole than for the purpose of compressing the joint. This is an important point, as otherwise an application, which almost invariably affords security and ease, may occasion pain with all its attendant mischief."

The objects of this treatment appear to be, first, to induce a gentle but general excited state of the cutaneous and superficial vessels of the joint; secondly, to prevent all motion; and thirdly, by this and by the mechanical support to abate the chronic congested state of the vessels. Sometimes the first object is not sufficiently obtained by the camphorated spirits, and then the tartar emetic ointment may be useful; and when the second indication is not fulfilled by the plasters, pasteboard splints are recommended. These dressings need not be removed more than once a week, unless some peculiar circumstances require it, such as the recurrence of heat or pain demanding local bleeding, or the discharge of matter, or the cessation of the superficial irritation.

This treatment is suited to every stage of articular disease, excepting that of acute inflammation, and has succeeded, according to Mr S., more frequently than any other plan, and that too in some of the worst forms and stages of the complaint—where the bones, cartilages and membranes are involved, sinuses formed, and severe constitutional irritation excited; the latter symptoms of course receiving suitable attention. Cases of this character are given.

Mr S. objects to the practice of puncturing the joint when distended with matter as advised by Mr Brodie. If pressure, as above recommended, does not answer for the dispersion of the pus, its escape by ulceration is a safer mode of termination. He objects also to stimulating injections into the fistulæ; if the bone is merely carious, not necrosed, it is unnecessary to cause an exfoliation which would not otherwise have occurred. If really dead, the removal by art would induce a more extensive exfoliation, than if left to the absorbents.

We hope these details will encourage American surgeons in the scientific treatment of these tedious but still remedial diseases.

34. *High Operation.*—At the sitting of the Section of Surgery on the 14th of February, and on the 13th and 27th of March, 1828, a very animated discussion occurred as to the advantages and dangers of the high operation for calculus. MM. Souberbielle, Roux, Larrey, Ribes, Dubois, Lisfranc, and Amussat were the principal speakers—men well acquainted with the theory and practice of surgery. The subject was introduced by a letter from M. S., who, our readers will remember, has of late years revived this operation, stating that his octogenarian patient (vide the last No. of this Journal, p. 191.) had recovered without any accident, and that this individual was the seventh cured consecutively by the method of Frere Come, without any perineal incision, or the introduction of an elastic catheter through the wound, or of strips of linen, or the employment of sutures in the bladder, as recently again recommended. This method is useless, and even aggravates the dangers. To prevent the urine passing by the wound, M. S. resorts to an apparatus which he terms *a compound syphon*, consisting of two or three elastic catheters attached one to the other by their extremities, and fitted to the catheter introduced per urethram into the bladder, after having filled them with water. In this way, the flow of urine is fully established even when it is thick or loaded with mucus, and all danger of urinary infiltration at the wound is prevented.

The discussion turned upon the points—whether the urine frequently escaped at the wound; and whether, under such circumstances, there was danger of urinary infiltration giving rise to inflammation, abscess, &c. M. Amussat, a graduate of 1826, after a few operations, delivered his opinions with great confidence, that, unless his method was adopted, it was physiologically impossible that the urine should not pass by the wound, that it was a want of good faith to deny this occurrence, that the bladder, as divided in the operation above the pubis, is no longer a complete pouch, and that the intestines, pressing upon this viscus, prevent the accumulation of urine in its cavity, and force this fluid through the wound. On the contrary, MM. Ribes, Gendrin, and Souberbielle assert that in eight patients operated on by the latter gentleman, the urine escaped from the wound only in one case. They had each examined the patients. Larrey, after much experience in forty years constant and varied practice, affirms that there is very little danger in the escape of urine from the bladder, as is constantly observed in wounds of this viscus: that this circumstance has absolutely no influence on the success of the high operation, and that instruments introduced into the wound are perfectly useless and often dangerous.

Some further discussion ensued on the 13th of March, when M. Roux reported the case of an ecclesiastic, aged seventy-two years, on whom he performed the high operation. He distended the bladder by injection, according to the method of Rousset, and was thus enabled to penetrate it without difficulty above the pubis. Two calculi were extracted and a catheter left in the urethra.

The urine flowed entirely through the wound, and until the fourth day no bad symptom ensued. Then the scrotum swelled; and a tumour formed at the root of the penis; these swellings suppurated rapidly: the patient became exhausted and died on the eighth day. *Post mortem*—no infiltration of urine was seen in the pelvis, but it had occurred in front at the inferior angle of the wound. The symphisis of the pubis was denuded, and a stone was found encysted at the inferior part of the right ureter.

The discussion turned on the necessity of introducing a catheter into the wound to carry off the urine. This M. AMUSSAT insisted upon; and DUBOIS observed, if this practice were proper, a very large catheter would be requisite so as to fill completely the opening in the bladder. This was denied by M. AMUSSAT, who maintained that the wound contracted exceedingly, so that no urine would escape at the side of the instrument. M. LARREY remarked that a catheter in the urethra would not remove the urine as rapidly as it arrives in the bladder, as its point would be too elevated. The instrument, he would say, should be as large as possible, and in some cases it might be proper to dilate the urethra previously to the operation, that it may admit a large catheter. The presence of the catheter causes no inconvenience, and his practice taught him that it was often sufficient to remove the urine as rapidly as it arrives in the bladder; and even were this not practicable, *the escape of the urine by the wound was altogether without injury*. M. AMUSSAT answered that it was "impossible" that the urine could escape through a catheter in the urethra after the high operation; because, when the bladder has contracted, the wound is in a more depending position than the mouth of the urethra: the solution of continuity is then very near the prostate, and the urine naturally flows towards the lowest point and passes through the wound or the canula which occupies the opening. The sitting of March 27 added no material facts to those above stated.

35. *Abscess within the Cranium cured by Trephining.*—M. ROUX reports (Jour. Général. de Méd. Avril, 1828) the case of a boy, fifteen years old, who, for four years, had a fistulous opening in the left parietal region, caused by an abscess from a blow on the head. Whenever the pus accumulated, comatose symptoms supervened. M. ROUX applied the crown of a trephine to the opening in the cranium, to facilitate the removal of necrosed bone, which he imagined was the cause of the mischief. No dead bone was discovered; the fistula penetrated the dura mater; this membrane was incised and an abscess thus exposed pressing on the brain. The pus being evacuated, a considerable depression was observed on the left middle lobe of the brain. The depression gradually disappeared; the orifice from the trephine filled up, and the patient recovered. This termination of the operation was altogether unexpected.

Several similar cases are upon record; and we feel not a little surprised that surgeons of the present day should any longer hesitate in applying the common principles for the treatment of abscesses and fistulæ to such complaints when involving the contents of the cranium. Why an abscess of the brain, increasing inflammation, and causing compression, convulsions, paralysis, coma, and death, should not be penetrated, and its contents discharged as in similar cases in other tissues or organs, and why the patient's chance for recovery should not be greater after the puncture has been made than when left to unassisted nature, has not been explained. We believe that the results of experience and the deductions of reason are decidedly in favour of a judicious scientific interference on the part of the surgeon.

Since writing the above, we have read in the Journ. Univer. for April, a case of abscess in the brain, treated by the justly celebrated DUPUYTREN, which we think confirms the observations already made. A man, aged forty-five years, entered the Hotel-Dieu on the 8th of June, having been delirious for fifteen days, in consequence of a blow on the head. A large wound, exposing the bone, existed in the forehead. Sensibility, power of motion, respiration, digestion, and the organs of sense were unimpaired. The pulse was natural, as also his excretions. There was no loss of memory; his delirium was characterised by an in-of the Academy of Medicine, read a report on the 8th of May, on an essay of M. **Serres**, physician at Uzés, entitled—"*On the cauterisation of the cornea, for correcting, in a prompt and sure manner, alterations of sight with dilated pupils.*" M. **Serres** proposes to treat idiopathic paralysis of the iris, without affection of the retina and optic nerve, by applying nitrate of silver to the cornea near its junction with the sclerotica. Four cases are given by him, and the committee of the Academy, having employed this remedy in three cases, bear testimony to its efficacy. The caustic should be applied for one second, and it is useful that some lachrymation should be excited and also a slight injection of the vessels of the conjunctiva. The light cloud, which appears on the cornea, vanishes in a few days.—*Archiv. Général. June.*

40. *Aneurism of the Brachial Artery cured by Pressure.*—M. **Tetu**, a regimental surgeon, details the particulars of a case of false aneurism at the bend of the arm, cured by pressure, which method he prefers to the ligature now so universally recommended. His patient in twenty-one days was cured—the tumour having disappeared, and the artery being obliterated for four inches. This is the second case in which he succeeded; the first patient recovered in sixteen days. His mode of compression is by a bandage methodically applied from the hand, by a graduated compress over the tumour and in course of the artery, and covered by a splint two inches wide and fifteen in length. Another splint, four inches wide and also fifteen long, was applied on the opposite side of the arm. Both were secured by a bandage, so as partially to interrupt the blood in the injured artery. In a few days, the pressure was increased, so as to arrest the passage of blood in the vessel; the circulation, however, continued in the anastomosing vessels, owing to the arrangement of the splints by which they were protected from compression.—*Journ. Univer. May.*

41. *Puncturing the Dura Mater.*—There are but few cases on record in which patients have survived when the dura mater has been punctured by a surgeon for the evacuation of effused blood; and most surgeons unite in opinion as to the danger of the operation. [Vide first volume of this Journal, p. 182, for a notice of the dangers, &c.] At the Medico-Chirurgical Society, Mr **Brodie** related the following case: A woman fell into a cellar, and pitched on her head. She lay senseless and apparently in profound coma or apoplexy. Mr **Ogle** examined the head after it was shaved with great care, but could discover no mark of fracture or other injury. When pressure, however, was made on a particular portion of one of the parietal bones, the patient appeared to evince some signs of sensibility. After employing the usual means, and waiting a reasonable time without symptoms of returning sensibility, Mr **Ogle** determined to apply the trephine over the part above mentioned. A portion of bone being removed, the dura mater of a blue colour bulged out, and was soon on a level with the external surface of the surrounding bone. The sense of touch convinced Mr **Ogle** that there was blood effused under the dura mater. He therefore punctured this membrane, and blood immediately spouted forth. The patient almost immediately recovered sensibility, and eagerly demanded "what they were about." She recovered.—*Medico-Chirurg. Rev. July*, 1828.

42. *Concussion.*—At the Medico-Chirurgical Society, two cases of concussion of the brain were detailed. In one the patient remained insensible for 17 days; in the other the insensibility continued *three weeks*; both patients eventually awoke, and recovered without a bad symptom. No treatment was resorted to.—*Medico-Chirurg. Rev. July.*

43. *Dissecting Wounds.*—The well known terrible consequences often resulting from these accidents—trivial in appearance—have been attributed by some to the absorption of a poison, and by others to a peculiar predisposition

of the patient's system. Mr A. COOPER appears, *at present*, to be inclined to the former opinion, from what we learn from the Med. Chir. Review for July last, although he has, if we mistake not, openly advocated the idea that a predisposition was requisite. He says, "It would seem under certain circumstances, a poison is produced sufficiently strong to excite inflammation, even when there is no wound."—"Mr COOK, surgeon, sent to me whilst labouring under the highest irritative fever, in consequence of having opened the body of a person who had died of puerperal fever. When I examined him, I found the extremities of his fingers of both hands inflamed, as if they had been dipped in scalding water, and the absorbents of his arms red, hard, and knotted to the axilla; yet he had not any wound or abrasion of any kind upon his hands, and it would therefore seem that the fluid produced in the abdomen of this woman, in which his fingers had been frequently immersed, was of a highly stimulating nature."—*Vol. III. Lect. Surg.*

44. *Treatment of Burns with Raw Cotton.*—The practice of treating burns with raw cotton, which originated in the United States, has been successfully imitated by Dr ANDERSON, who gives the results of his experience with it in a paper published in the Glasgow Medical Journal for May 1828.

Dr ANDERSON states, that the most striking effect of the cotton is to allay pain and irritation, rapidly and altogether, in all burns, whether characterised by simple vesication, or deep disorganization and suppuration. Some of his patients, who had been previously treated by the old method, bore marked testimony to the advantages of the cotton in relieving the pain; and he remarks that even in hopeless cases, in consequence of the extent of the injury, the application of the cotton is speedily followed by great relief, and even a total subsidence of suffering. The consequence of this is, that the heat of the body diminishes, the anxiety ceases, sleep is restored, and the appetite returns.

By thus checking inflammatory action, the cotton in superficial burns always accelerates the cure, often completing it, without leaving any material scar. Dr ANDERSON conceives, that the cotton forms, with the discharges, a substitute for the epidermis, and thus affords a protection, which enables the process for the formation of the new cuticle to take place, undisturbed by external irritations. In the case of a collier, who was extensively burnt, the cotton was allowed to remain undisturbed for fourteen days; and when the mould of matted cotton was removed, it was found that many inches of the circumference of the sores were healed, their centres presenting, not granulations, but the denuded true skin, with its papillæ inflamed and enlarged, so as to present a granular appearance. In this case, the patient got rid of the marks of small pox.

Dr ANDERSON made a comparative experiment on a girl, who had both legs similarly and severely burnt. The leg dressed with cotton gave her little uneasiness, and when the cotton was removed at the end of the third week, a new and sound skin presented itself. The other leg which was dressed with the linimentum calcis, continued long inflamed and acutely painful, and several troublesome ulcers resulted, which were not entirely healed until three months had elapsed.

When a burn is severe and deep, it is known frequently to kill the true skin and parts underneath, and the consequent slough is usually a long time in being separated. Frequently the patient sinks under the violence of the inflammation or the profuseness of the discharge, or if he escapes, the cicatrix is usually contracted. Dr ANDERSON conceives that these evils are greatly mitigated by the use of cotton, and sustains his assertion by the history of a very severe case of burn in which cotton was the principal remedy, and which ended in recovery.

Dr ANDERSON directs, that the cotton should be finely carded in narrow fleeces, thin enough to be translucent. If vesications exist, the serum is to be let out, and the parts washed with tepid water. If the skin is deeply scorched, a spirituous or turpentine lotion is better. The cotton is then applied in successive layers, until the injured parts are completely protected from the effects of motion and pressure; and a bandage may sometimes be necessary to retain it.

When the discharge has soaked through the cotton, or become insupportably offensive, the layers should be removed as far as can be done without exposing the injured parts, and fresh layers substituted. The first dressings should be allowed to remain undisturbed as long as possible.—*Edinb. Med. and Surg. Journal for July, from the Glasgow Med. Journal.*

VII. MIDWIFERY.

45. *Cæsarian Section.*—Mr JOHN CRICHTON performed this operation, in April 1826, on a female aged 22 years, whose pelvis was greatly deformed in consequence of a severe injury sustained at a threshing mill some years previously. Labour had continued for several days, and fatal symptoms had appeared; there was reason to believe the child was living. The operation was readily performed, and the child removed in a state of asphyxia, but was soon revived. The mother however died in a few hours. A portion of the scalp of the child spacelated from the long continued pressure on the pubes, nevertheless he throve well, and is now (May 1828) a stout healthy boy.—*Ed. Med. and Surg. Jour. July.*

46. *Retention of the Placenta.*—In RUST's Magazine is related a case by Dr HOFFMANN, in which the practice proposed by M. MOJON, of injecting the vessels of the placenta with fluids to promote its expulsion in case of retention (vide the second volume of this Journal, p. 438) was successfully imitated. In this instance, after delivery of the child, the placenta remained adherent, the os uteri contracted, the abdomen and uterus enlarged, while the paleness, faintness, and the loss of pulse indicated internal hemorrhage. The injection of some warm water and spirits was in one minute followed by the contraction of the uterus, and subsequently by the expulsion of the placenta. The patient recovered rapidly.—*Jour. des Progrès, Vol. IX. p.* 233.

This practice is exciting attention. A long essay, including the detail of cases, has been presented to the Medical Society of Paris, at its meeting in January last, and was reported on very favourably by MM. DUCHATEAU, CHAILLY and GENDRIN. The essay and report have been published in full in the April No. of the Jour. Gén. de Méd. The reporters close their labours by observing, "that the memoir of M. LEGRAS is well prepared. The author has endeavoured to restrict himself to actual experience, and he appears to us to have demonstrated by facts the safety in all cases of injections into the vessels of the cord after the birth of the infant, their efficiency in causing a separation of the placenta, in arresting hemorrhage from a partial detachment of this body, and finally in stimulating the uterus in cases of inertia of this organ. The views we have taken, and the experiments we have reported, conduct also to this important practical fact, that the effect of the injections can be regulated by graduating the temperature and quantity of the fluid injected, by rendering it more or less styptic, and by augmenting, at pleasure, the impulse by which it is thrown into the vessels. If a few ounces (say ℥vi.) of fresh water be adequate for the separation of the placenta in ordinary cases, it would be imprudent to trust to this quantity in dangerous hemorrhages from partial detachment of the placenta with absolute inertia of the uterus. In such cases we should rapidly fill and distend the placental vessels, so that the cold and styptic fluid should be driven even to the uterine surface, by thus forcing it to exude (*suinter*) from the placenta."

47. *Abortion.*—In the 9th volume of the Journal des Progrès, p. 237, is related a case of abortion, at the end of the third month of pregnancy, in which the placenta, with one inch of the umbilical cord attached to it, was expelled twelve days before the delivery of the fœtus. The placenta had the size of one belonging to a fœtus of between two and three months; it was perfectly healthy,

had no unpleasant odour, and was perfect in its form. Its expulsion was preceded and accompanied by much hemorrhage and pain, which soon after ceased, so that the patient appeared quite well, resuming her strength, appetite, &c. At the expiration of twelve days, a bloody fœtid discharge came on per vaginam, with pain, chills, &c.; followed after some hours by the delivery of a fœtus, two inches in length, and with a fragment of a cord eighteen inches long, in a state of putrefaction. The age of the fœtus, probably not more than seven or eight weeks, did not correspond with that of the placenta; and hence there is reason to believe that at the end of the eighth week, a rupture of the cord took place, and the consequent death of the child, while the placenta continued to grow until expelled during the third month. Why the placenta should have been thrown off before the fœtus, a fact of very unusual occurrence, and how the infant was retained, must remain a subject of speculation.

48. *Hysterotomy.*—M. Kluge performed this operation at Berlin on the 24th of August, 1827. The patient was twenty-three years of age, and greatly deformed from disease of the spine, pelvis, &c. Labour supervened at the full period of pregnancy and delivery being impracticable, the Cæsarian operation was performed. The child survived. The mother, a few hours after the operation, was affected with spasms of the muscles of the neck and thorax; with rapid and hard pulse. On the 25th, the nervous symptoms, the hard and frequent pulse increased; vomiting and convulsions supervened with febrile symptoms, &c. She died on the 28th. "*On opening the body no traces of inflammation or of gangrene could be discovered in the whole extent of the intestinal canal, in the peritoneum, in the uterus, nor in any other organ.*" The edges of the external wound did not adhere, neither did those of the incision in the uterus. The lungs, the structure of which was healthy, contained a very small quantity of blood.—*Journ. des Progrès, Vol. IX. p.* 264.

49. *Extirpation of the Uterus by Ligature.*—This operation was lately performed in London on a lady who two years previously suffered an inversion of the uterus from a forcible removal of the placenta, and for a considerable period was sinking from occasional hemorrhage. A ligature was applied and occasionally tightened. At the end of a fortnight, the tumour, formed of the uterus, came away; and at the end of another week (the time of the report) there were no bad symptoms; the patient however continuing weak and pale.—*London Med. and Surg. Journ. for July.*

VIII. CHEMISTRY AND PHARMACY.

50. *Action of Muriatic Acid on Albumen.*—In the "Journal de Chimie Médicale" for July, 1828, M. Bonastre gives a note on the effect of this acid on different varieties of albumen. He confirms the previous remark of M. Caventou, that the addition of pure muriatic acid to albumen gives rise to a blue colour, although several of his colleagues, on repeating the experiment, did not succeed in producing the phenomenon.

M. Bonastre has succeeded in producing a blue colour with the following specimens of albumen; namely, the white of eggs, the crystalline lens, and the vegetable albumen existing in the seeds of the *dolichos urens*, the *misosa scandens*, and *phaseolus vulgaris*, or kidney bean.

If any sure and easy method for applying this test could be devised, it would be valuable as a means of detecting animal and vegetable albumen in solutions.

51. *Analysis of Cancerous Matter.*—In the Journal de Chimie Médicale for July, 1828, M. Collard De Martigny gives an analysis of this matter, from which we furnish the following abstract:—

Physical Properties.—A white tissue of a nauseous smell, and lobulated appearance, resisting the scalpel but slightly, and heavier than water. When dried, it was yellowish brown, of a faint odour, slightly brittle, presenting a filamentous and whitish fracture, and semi-transparent.

Chemical Properties.—Half an ounce of the matter, exposed to the air, after having been allowed to remain in water for several hours, became conical, soft, and almost pultaceous. Its smell was strong and nauseous, but of a different character from that of the fresh matter, being less virulent (vireuse). The colour was various, being of a bronze brown on the sides, and yellowish blue at the summit.

Treated with cold water, a slight portion of albumen was dissolved, which precipitated by heat in the form of frothy flocks.

On distillation, it gave no volatile principle. Ether and alcohol, in the cold, had no sensible effect on it.

On being boiled in distilled water, this menstruum dissolved a considerable quantity of gelatin, which was white and perfectly transparent. The insoluble residue was grayish-white, devoid of any fibrous appearance, and soft, and presented all the characters of albumen.

Boiled with alcohol, the matter assumed a beautiful dull white colour, became very elastic and difficult to tear, smooth externally, and fibrous and filamentous internally. The alcohol acquired a greenish colour. Upon being filtered and allowed to cool, it became whitish and milky, and let fall a pulverulent precipitate, which disappeared upon re-heating the alcohol. When evaporated, it assumed a denser and deeper emerald-green colour, and when dryness was attained, a substance was left of a deep yellow colour, and agreeable smell, possessing a medium consistence, and which stained blotting paper, and burned like a fatty body. This matter, which had a decided acidity, and was insoluble in caustic potassa, is considered by M. COLLARD to consist of cerebral fatty matter.

M. COLLARD sums up the results of his analysis as follows:—

Albumen	0.206
Gelatin	0.021
Fatty matter	0.020
Phosphorus and salts	Trace
Water.	1.700.

52. *Coniin, or Active Principle of Cicuta.*—M. BRANDES gives the following process for obtaining this principle:—Digest the leaves and stem of the fresh plant, well bruised, for several days, in alcohol. Filter the solution, and evaporate to dryness. Treat the alcoholic extract with water, and add to the aqueous solution obtained, either magnesia, alumina, or the oxide of lead. Evaporate this solution to dryness, and treat the dry residue with a mixture of alcohol and ether. This menstruum takes up the coniin, which, by a new evaporation to dryness, is left in a pure state.

This principle, according to M. GISEKE, possesses the following properties, by which it may be distinguished:—1st. In contact with tincture of iodine, its solution gives rise to a reddish precipitate. 2d. Tincture of galls renders its solution brown, but causes no precipitate. 3d. It precipitates solutions of the sulphate of mercury and muriate of zinc, of a dirty yellow colour. 4th. It occasions a slight turbidness in solutions of the carbonates of potassa and soda. 5th. It communicates a brown colour to the muriate of platinum. 6th. With the nitrates of silver and baryta, the acetates of baryta and lead, the muriate of lime, and lime-water, it gives rise to grayish-white precipitates.

Half a grain of coniin is sufficient to kill a rabbit. The symptoms induced by it are analogous to those produced by strychnia. After death, the vessels of the head, the right auricle of the heart, the superior vena cava, and the jugular, are very much gorged with blood, while the abdominal vessels appear to be completely empty.—*Archives Générales, June* 1828—*from Arch. Brandes, Vol. XX.* 1827.

53. *Red Ferro-cyanide of Potassium.*—This substance was discovered by GMELIN, and has been proposed by BERZELIUS as a reagent in the examination of mineral waters, as it detects the presence of proto-salts of iron by a green or blue precipitate, while it has no action on the per-salts of the same metal. It is formed by passing a current of chlorine through a solution of the ferro-prussiate of potassa, until this salt no longer precipitates the per-salts of iron, and then filtering the solution, and allowing it to undergo a slow evaporation. This cyanide is in the form of reddish-yellow needles, exhibiting the metallic lustre. According to BERZELIUS, it contains one and a half times as much cyanogen as the ordinary ferro-cyanide of potassium (ferro-prussiate of potassa). M. GIRARDIN has ascertained that it dissolves in twice its weight of cold water, and in its own weight of boiling water. It is not soluble in absolute alcohol. It greens the sirup of violets. As a reagent for iron, it is more sensible than the ferrocyanate of potassa, since it will detect this metal, when present, in solutions four times more dilute than those in which the latter salt can detect it.—*Archives Générales, June* 1828.

54. *Dangers from the different Strengths of Pharmaceutical Preparations of the same Medicinal Agent.*—In the Journal Général de Medicine for June 1828, the editor, Dr GENDRIN, gives an account of the shocking occurrence of the death of seven patients at the hospital of the Bicêtre at Paris, in consequence of their taking a stronger preparation of the hydrocyanic acid than was contemplated by the physician.

It appears that the physician of this hospital, Dr FERRUS, having derived advantage from the administration of hydrocyanic acid in epilepsy, was desirous of giving it a trial at the hospital. He had been in the habit of prescribing it in the form of sirup, as prepared by M. PELLETIER, after the directions given by MAGENDIE in his formulary. The dose, with which Dr FERRUS was in the habit of beginning with this preparation, was half an ounce, as laid down by MAGENDIE.

Dr FERRUS, accordingly, chose as the subjects of his experiments fourteen epileptic patients of the Bicêtre hospital, from the age of fifteen to thirty years. For each of these, he prescribed the sirup of hydrocyanic acid, which, not being in the shop of the hospital, was ordered to be procured from the magazine of the central pharmacy of the hospitals. The medicine having arrived, Dr FERRUS expressed his intention to begin with an ounce of the medicine; as he was satisfied from experience, that this dose was devoid of danger. The hospital apothecary, however, having remarked, that it was not customary in the practice of the house to exceed a drachm of the sirup in twenty-four hours, and that even this dose of the medicine, as prepared at the central pharmacy, was sometimes too powerful, Dr FERRUS confined himself to the prescription of half an ounce of the sirup, to be taken at once. Accordingly, on the 17th of May, half an ounce of the sirup of hydrocyanic acid, as obtained from the central pharmacy, was commenced to be given to each of the epileptics. The nurse had already administered it to seven of the patients, when he observed that one of them was rendered ill. This induced him to examine the others, when he found the first already dead, the second just expiring, and the third exhibiting all the symptoms of being poisoned. At the end of fifteen or twenty minutes, the seven patients were all dead!

It may be easily imagined that so shocking an occurrence in a large establishment like the Bicêtre produced a great sensation in Paris. Public clamour spread the facts, which very soon came to the knowledge of the judicial authorities; and an inquest was immediately instituted, by the king's attorney, to investigate the circumstances. It was ascertained that the nurse had made no mistake, nor had any been committed in the preparation of the article, at the laboratory of the central pharmacy, it having been prepared precisely according to the formula of the French Codex.

The shocking catastrophe plainly arose from the circumstance, that an active medicine was prepared under the same name, of such opposite strengths, as to constitute, in the same dose, a medicinal article and a most deadly poison. Dr Ferrus had been in the habit of prescribing Magendie's sirup in his city practice, and by a fatal mistake the sirup of the Codex was given at the hospital.

Dr Gendrin states that the sirup of the French Codex consists of simple sirup, nine parts, and medicinal hydrocyanic acid, one part; while Magendie's formula directs, clarified sirup of sugar, one hundred and twenty-eight parts, medicinal hydrocyanic acid, one part. It is perceived, therefore, that while the acid constitutes a tenth part of the sirup of the Codex, it forms only the one-one hundred and twenty-ninth part of that of Magendie. Every half ounce of the Codex sirup contains 57.6 grains of medicinal hydrocyanic acid, while the same quantity of Magendie's sirup contains only 2.24 grains; so that it appears that the unfortunate patients of Dr Ferrus took about twenty-five times as much medicinal hydrocyanic acid at a dose, as he had intended to give them.

Dr Gendrin proceeds with some criticisms on the formulæ for hydrocyanic acid of the French Codex. It appears that the hydrocyanic acid of this official pharmacopœia is prepared according to two formulæ, one being designated as that of Robiquet, the other as derived from Vauquelin. But Dr Gendrin affirms that Robiquet's formula is not correctly followed: for while he directs the anhydrous acid to be diluted with *twice* its volume of water, the Codex directs it to be mixed with its own volume! But even Robiquet's acid is stronger than that of Magendie, which consists of one measure of acid to six of water.

Supposing the syrup of the Codex prepared with the strong acid, erroneously said to be according to the formula of Robiquet, half an ounce of it would contain nearly twelve grains of pure anhydrous hydrocyanic acid. It may be easily imagined what would be the action of such a dose!

Dr Gendrin states, however, that the acid obtained for administration at the Bicêtre was not so strong as that above indicated. The pupil at the laboratory of the central pharmacy prepared it according to a formula, which had recently been published in the Journal of Pharmacy. By it, he obtained an acid stronger than 0.90, the density directed in the Codex. He accordingly diluted it with water, and for the sake of greater precaution, brought it to 0.92, or weaker than directed by the Codex. It was with an acid of this strength that the fatal sirup had been made. Every half ounce of it contained nearly eight grains of pure acid.

Dr Gendrin next mentions the other formula of the Codex, or that of Vauquelin, which contains about 21 parts of pure acid to 800 of water, and which is, therefore, weaker than Magendie's acid, and consequently still more so than that of Robiquet. Dr Gendrin speaks in terms of disapprobation of the framers of the French Codex, for having so carelessly and unwarrantably directed the same medicinal article to be prepared by different formulæ, which produce it of different strengths; for, says he, a patient who could take a given dose of Vauquelin's acid without danger, would be infallibly destroyed by the same dose of acid of the density of 0.90, or even 0.95, or by half the quantity, when prepared by the Codex formula, erroneously attributed to Robiquet.

Here perhaps Dr Gendrin goes too far in his strictures on the Paris Codex. We have not the work before us, but it seems probable, from what Dr G. himself states, that the Codex, in directing a particular specific gravity (0.90), had in view that the acid should always be brought to that strength. Hence, though there are different formulæ laid down, which furnish products of different strengths, yet the acid is not the acid of the Codex, unless it have the specific gravity of 0.90. This difficulty, however, occurs, that Vauquelin's acid weighs as heavy as 0.98: now it may be very easy to dilute the acid if too strong, but it is not so easy to abstract a portion of water, in case it should be too weak.

Dr Gendrin sums up his criticisms in the following words. "Thus in whatever point of view we consider the formulæ of the Codex, they are in all respects erroneous. They are erroneous in the density stated of the acid of Scheele; erroneous in the preparation of the hydrocyanic acid of Robiquet, in ordering but half the water directed by this pharmaceutist; erroneous in assimilating the acid of **Robiquet** to that of **Scheele**, which is three times as weak, and to that of **Vauquelin**, which is still weaker; and finally, erroneous in directing a quantity of acid in the cyanic sirup beyond all reasonable proportion."

Dr **Gendrin** gives an extract from the formulary of **Magendie**, by which it appears that this distinguished physician pointed out the dangers of these formulæ of the Codex, and even foresaw the disastrous consequences likely to ensue from a preparation of such inordinate strength as the cyanic sirup. His words are, " This sirup, thus prepared, could only be exhibited by drops; for if, by any mishap, an ounce was mixed in any potion, it would prove certainly mortal."

The bodies of the seven unfortunate patients were opened by Drs **Adelon** and **Marjolin**. They did not find any lesion which could be assigned as the cause of the epilepsy. The heart was empty, and the blood in the vessels not coagulated. This liquid was found congested in the vessels of the brain and abdomen. The bodies did not exhale any odour of bitter almonds.

55. *State of Pharmacy in France.*—The great attention which is paid to the instruction of pupils in the science of pharmacy in France may be learned from the following account of an examination of candidates, for the places of pupils in pharmacy to the hospitals of Paris, which we condense from the Journal de Chimie Médicale for July 1828.

Count **Chaptal** presided at the examination, assisted by Drs **Jadiaux** and **Lallemant**, M. **Henry**, chief of the central pharmacy, and MM. **Duval**, **Harveng** and **Petroz**, chief pharmaceutists of the hospitals. The exercises consisted of three kinds: 1st. Questions to which written answers were required; 2d. Questions to be answered verbally; and 3d. Manipulations, which were performed before the board of examiners. The questions to be answered in writing were, 1st. What is gum arabic? describe the kinds in commerce and state their origin. 2d. State the preparation of citrin ointment, its characters, and the alterations which it is liable to undergo by time, &c. 3d. What is the chemical composition of opium? give the methods of extracting morphia and narcotin. The verbal questions related to the different kinds of distillation;—the nature of sirups and their mode of preparation;—the menstrua for separating the soluble parts of plants;—the nature of conserves, and the general rules of their preparation;—the distinctions between cerates, pomatums, and ointments, and the general rules for their preparation. The manipulations required to be performed were 1st. To make an emulsion with turpentine;—2d. To prepare whey.

The examination having terminated, the written answers of the candidates were considered at seven meetings. The relative merits of the pupils were discussed by the aid of the notes taken by the examiners, and each was classed according to his proficiency.

Finally, the board of examiners, at a public sitting, announced the names of those candidates who were deemed worthy of being recommended as resident pupils, and of those also, who were to be considered as provisional pupils, to fill vacancies until the next examination.

A course of examinations of this kind, in which, not merely the absolute, but the relative acquirements of the candidates are ascertained, must be viewed as well calculated to call forth the greatest exertions on the part of the pupils, on the principle of rivalship. It deserves to be imitated, as far as possible, in the schools in which pharmacy is taught in the United States.

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